

NATURAL SCIENCE

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NOTES AND COMMENTS

POLAR EXPLORATION

THE balloon voyage of Andrée in the Arctic Regions seems to have monopolised public interest this month, and the absence of news from him has caused some very unnecessary anxiety as to his safety. Even if the winds be favourable and the balloon do not leak, he is not due on the coast of either America or Asia until well into August; and it is then quite possible that he may have to spend the winter in some northern post, from which news may not reach Europe until the spring. Several other important expeditions are in the field. Lieutenant Peary is leading another party to Greenland, where he hopes to make arrangements for his proposed Polar expedition and to secure the great block of meteoric iron which could not be removed last summer. He will be accompanied by Mr Charles Schuchert and Mr White, who hope to make large collections of the famous fossil plants of Disco Island. Sir Martin Conway and Mr Garwood are continuing the exploration of Spitzbergen by crossing the northern ice-sheet on ski. The *Windward* has again sailed for Franz Josef Land, and ought to be back in September with news of Mr Jackson's latest achievements. Meanwhile but little has been done in Antarctic research. The Belgian expedition under Dr Gerlache and M. Arctowski have at length obtained the necessary funds, and left Europe in the middle of August. The proposed German expedition is still appealing for support, but does not appear to be very warmly taken up. We hope, however, that the Royal Geographical Society will see its way to a vigorous agitation during the winter in favour of its proposed British Antarctic Expedition.

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ARCTIC GEOLOGY

REGRET at an ignorance concerning Antarctic geology is increased by two papers in *Nature*, in which Dr J. W. Gregory reminds us

of the interesting problems connected with the history of the Arctic regions. In the first paper, a summary is given of the geological structure of the land masses surrounding the Arctic Ocean; the variations in the relative positions of land and water are traced, and it is argued that the Polar Basin has been formed by subsidence during Tertiary times. In the second paper, the author considers the changes in climate that have taken place in the North Polar Regions. He refers to the famous theory according to which the Arctic regions were once clothed in tropical vegetation and their shores were once fringed by coral reefs. The evidence on which this theory rests is, however, shown to be very untrustworthy. The plant determinations made by Heer are unreliable, and there is no evidence that coral reefs were ever formed within the Arctic Circle. Corals grew in Arctic seas in earlier times as they do to-day, but there has been no adequate proof that they ever formed reefs. Dr Gregory accordingly distrusts all the theories as to the great size of the sun in Palaeozoic times and the universal uniform climate in the pre-Tertiary period, which have been based on the asserted Arctic palm-groves and coral seas. That climatic changes have occurred is not disputed, but the author does not think it possible to estimate their extent until the palaeontology of the Arctic regions has been carefully revised. The most important work on this subject now being carried on is Professor Nathorst's redescription of the fossils about which Heer theorised so wildly. Dr Gregory also concludes that palaeontological evidence tells strongly against the view that the position of the Poles has altered to any great extent.

BIRDS AND THEIR STOMACHS

THE United States Department of Agriculture, knowing that the welfare of the country depends largely on the prosperity of the farming class, has undertaken for long past a proper consideration of birds in their relation to agriculture. In its fifty-fourth bulletin it deals with the stomach-contents of some twenty common birds. Among these may be mentioned the cuckoos, woodpeckers, bluejays, ricebirds, blackbirds, orioles, cedarbirds, catbirds, bluebirds, &c. There is a good deal of practical common-sense in the introduction of this pamphlet by Mr F. E. L. Beal, who points out the tendency to dwell on the harm done by birds rather than the good. He goes on to say :—

“ Within certain limits, birds feed upon the kind of food that is most accessible. Thus, as a rule, insectivorous birds eat the insects that are most easily obtained, provided they do not have some peculiarly disagreeable property. It is not probable that a bird

habitually passes by one kind of insect to look for another which is more appetizing, and there seems little evidence in support of the theory that the selection of food is restricted to any particular species of insect, for it is evident that a bird eats those which by its own method of seeking are most easily obtained. Thus, a ground-feeding bird eats those it finds among the dead leaves and grass; a flycatcher, watching for its prey from some vantage point, captures entirely different kinds; and the woodpecker and warbler, in the tree tops, select still others. It is thus apparent that a bird's diet is likely to be quite varied, and to differ at different seasons of the year.

"In investigating the food habits of birds, field observation can be relied on only to a limited extent, for it is not always easy to determine what a bird really eats by watching it. In order to be positive on this point, it is necessary to examine the stomach contents. When birds are suspected of doing injury to field crops or fruit trees, a few individuals should be shot and their stomachs examined. This will show unmistakably whether or not the birds are guilty."

In his notes on the tree-sparrow (*Spizella monticola*) Mr Beal shows that the stomachs of these birds in winter are crammed with the seeds of weeds, and he estimates that in the State of Iowa alone, if there are only ten birds to a square mile, no less than 875 tons of weed seed are consumed by this single species in a single season, basing his calculations on the modest estimate that each bird eats one fourth of an ounce a day for a winter season of 200 days. This may be used as an argument by the ignorant to show how much they eat of grain in the summer, but examination of stomachs of the same birds in summer shows conclusively that one third of the bulk is made up of insects (not available for consumption in the winter), grass and weed seed, and a little oats. The young birds also are largely fed on insects.

We cannot spare space to quote the statistics of other birds, but the story is much the same in each case. It is for the farmer to decide whether he cares to spare a little grain in the summer in order that his fields may be kept comparatively free of weeds from year to year, or whether he prefers to kill the birds and have his pockets emptied by paying for weeding, and the destruction of hosts of insects which are kept at bay solely by the birds he so religiously endeavours to destroy.

We have already one work on the economic ornithology of Great Britain on the lines of this bulletin ("Ornithology in relation to Agriculture and Horticulture," by various writers, edited by John Watson, 1893); but a real benefit would accrue to the farmers in enabling them to know accurately their friends and their enemies

(should they care to do so), if the Government or the County Councils would take up the question officially and systematically. But such a work would involve much original research.

AMERICAN ECONOMIC ENTOMOLOGY

WE have also received from the U.S. Department of Agriculture a short but interesting pamphlet by Mr C. L. Marlott on "Insect Control in California." The well-known plan of introducing lady-bird beetles to prey upon the imported scale-insects which devastate the western fruit-orchards has been successfully extended; while an efficient artificial insecticide has been found in hydrocyanic acid gas with which the trees are fumigated after being covered with a temporary canvas tent. Mr F. H. Chittenden writes on the European Asparagus beetles which, like so many old-world insects, have been introduced into the Atlantic States. Dr L. D. Howard gives an illustrated account of various portable steam pumping-engines used for spraying trees with insecticide fluids.

GLANDS IN INSECTS

In the latest part of the *Transactions* of the Entomological Society of London (1897, pp. 113-126, pt. v.), Mr Oswald H. Latter describes the structure and function of the sternal gland found in the prothorax of the caterpillar of the "Puss" moth (*Cerura vinula*). The formic acid secreted by this gland has long been recognised as a defence to the larva against its enemies. Mr Latter has now shown that at the end of larval life the secretion has another function. Mixed with the silk the acid serves to make the cocoon which contains the pupa exceedingly hard and waterproof as well as strongly adherent to foreign substances such as the chips of wood which this caterpillar habitually works into its cocoon.

Mr Latter points out that in other lepidoptera and insects of different orders, many segments of the body possess glands which may reasonably be considered serially homologous with that under consideration; he suggests that all these glands represent the coxal glands of arachnids. The prothoracic gland of *C. vinula* opens into a shallow vestibule, whence arise branched eversible tubes bearing groups of spines in their cavities. Mr Latter is unable to suggest a satisfactory function for these tubes, but he points out that the groups of spines recall the parapodial setae of chaetopods, and that the whole structure supports Mr Bernard's view that such glands are derived from the acicular gland sacs of ringed worms. Should these relationships prove to be correct, Mr Latter believes that they "will go far towards establishing the primitive nature of the cruciform larva of which many observers are already in favour."

THE PHOTOGRAPHY OF MICROSCOPIC ORGANISMS IN MOTION

ACCORDING to the *Scientific American*, the principles of the kinetoscope have been applied to the microscope with some interesting results by Dr Robert L. Watkins of New York. The instrument employed, termed a micromotoscope, has been very difficult to devise, owing to the manipulation of the light and lens. When the light is concentrated sufficiently for photography, it very quickly kills or seriously injures almost any kind of life in the microscopic field. The greater the magnification, the more intense and the nearer the lens the light must be. Difficulties are also multiplied by the length of time sometimes taken in arranging the focus on the sensitive film. After repeated efforts, however, Dr Watkins has obtained some measure of success, and motions that are not too rapid have been very satisfactorily recorded. He has been able to produce about 2500 pictures per minute. This is not a sufficiently rapid process to photograph the motion of the blood circulating in the web of a frog's foot; but it has served admirably in the case of at least one rotifer, which exhibits the most interesting form of cell motion yet reproduced.

THE GREAT AUK IN IRELAND

REMAINS of the extinct Great Auk (*Alca impennis*) have already been recorded from the north of Ireland, but the known range of this interesting bird has just been considerably extended by the discovery of a few bones in a Kitchen Midden on the coast of Waterford, nearly as far south as 52° N. latitude (R. J. Ussher, *Irish Nat.*, vol. vi., p. 208). A humerus, tibia and metatarsus have been identified by Dr Hans Gadow and Professor Alfred Newton. They were associated with bones of common domestic animals and the red deer, and thus probably do not date back to an earlier period than the remains already found in the refuse-heaps of Caithness and Durham.

EXTINCT BIRDS OF MADAGASCAR

DURING his stay in Madagascar Dr Forsyth Major spent several months in the Sirabé district searching for remains of *Aepyornis*. What success attended his efforts has already been noticed in these columns, but besides *Aepyornis*, Dr Major discovered remains of numerous other birds associated with it. Mr C. W. Andrews, to whom we are indebted for the careful description of these *Aepyornis*

remains, is now collecting in Christmas Island, but before setting out on this expedition he left for publication a paper which has just appeared in the *Ibis* (July), dealing with the most remarkable of these other Madagascar birds. Those here described are mainly carinate and were associated with *Ae. hildebrandti* of Burckhardt, in a marly layer indicating an old lake bottom at a depth of 12 to 15 feet. Above the marl comes a coarse gravel consolidated with carbonate of lime and containing rolled and broken bones, which may mark a volcanic outburst accompanied by hot springs charged with that mineral. Above this deposit is another of black earth from 5 to 6 feet in thickness, in which bird bones occur though rarely. It is interesting to note that *Ae. hildebrandti* does not occur in the black earth, but remains of the smaller *Ae. mulleri* were found together with well-preserved bones of *Mullerornis agilis*.

The most important of Dr Major's discoveries as described by Mr Andrews may be briefly enumerated. A large Anserine bird, having resemblances to *Chenalopez pugil*, from Lagoa Santa, Brazil. This has been called *Centronis majori*. Another Anserine is closely allied to *Chenalopez aegyptiacus*, but the numerous slight differences between the fossil and the recent species induced Mr Andrews to term it *C. sirabensis*. He however thinks it possible that when further remains are found, it may turn out to be *Sarcidiornis mauritianus*, an extinct bird described by Newton and Gadow from Mauritius. A new rail, *Tribonyx roberti*, is described from a pelvis; while a well-preserved tibia is also referred to this species. *Ardea*, *Platalea*, *Astur*, and *Plotus*, are among the other remains discovered, but at present the material is not of a sufficient quantity to justify further description. It may be as well to note, however, that *Centronis* is described from remains of four or five individuals; and the *Chenalopez* from a large collection of bones, many of which were found associated.

THE ORIGIN OF THE EDENTATE MAMMALS

THE phylogeny of the edentate mammals has long been a standing puzzle to palaeontologists, and this gap in our knowledge has rendered it impossible to come to a full understanding of the South American fauna. So far as the typical or American forms (sloths, ground-sloths, ant-eaters, and armadillos) are concerned, the problem has been solved by the labours of Dr J. S. Wortman, of the American Museum of Natural History, of which an illustrated account has appeared in the *Bulletin* of the Museum (vol. ix. pp. 59-110). A valuable illustrated article on the same subject, by Prof. O. C. Marsh, has also been published in the *American Journal of Science* (vol. iii., 1897, pp. 137-146).

For many years there have been known more or less imperfect remains of certain remarkable and puzzling mammals from the Eocene of the United States, which have been described under the names of *Hemiganus*, *Psittacotherium*, *Calamodon*, and *Stylinodon*; the two first being from the Puerco beds, while the third is from the Wasatch, and the fourth from the Bridger and Wind river. The unfortunate animals to which these bones and teeth belonged have been shifted about from place to place, according to the fancy or bias of each individual describer; one of their last resting-places being among the Tillodontia.

Dr Wortman has, however, succeeded in showing that whereas in the latter it is the second incisor in each jaw which (as in the rodents) undergoes hypertrophism, in the animals forming the subject of his memoir it is the canine which undergoes special enlargement. Obviously, therefore, there can be no intimate relationship between the two groups; and as the one he has specially investigated requires a new title, the name Ganodonta has been proposed.

To enter into details of the structure of these ganodonts would obviously be out of place here. But any competent anatomist who may take the trouble to consult the excellent descriptions and figures given in the original memoir can scarcely fail to be convinced that in these animals Dr Wortman has succeeded in identifying the long-missing ancestors of the American edentates. Although the Puerco forms have enamelled and rootless molars, in the latter types the roots at first become confluent, and finally disappear, while at the same time the enamel becomes restricted to bands, and the whole structure of the tooth is simplified. The canines, too, become more and more like those of the Pliocene and Pleistocene ground-sloths; while the resemblance between the skulls and limbs of the latter and these of the ganodonts is such as to render no other conclusion possible but that the one group is the forerunner of the other. Not only, therefore, have the ancestors of the true edentates been discovered, but we have proof that the first tooth of the modern sloths is a canine, and not a premolar.

The Ganodonta are regarded as forming a sub-order of the Edentata; the genera mentioned above constituting one family (*Stylinodontidae*), while a second family (*Conoryctidae*) is made up of the genera *Conoryctes* and *Onychodectes*, to which further allusion is unnecessary in this place. Whether the living Old World families (*Orycteropodidae* and *Manidae*) should or should not be included in the Edentata, Dr Wortman leaves an open question; but in either event he confesses himself unable to draw up a satisfactory definition of the order.

THE SOUTH AMERICAN EDENTATE MAMMALS

HAVING satisfactorily demonstrated the ancestral position of the Ganodonta to the modern American Edentata, Dr Wortman goes on to observe that if this be true, "it follows that all the South American edentates must have been derived from the North American Ganodonta, since their earliest appearance in South America does not antedate the Santa Cruz epoch. In this formation they appear suddenly in great numbers and variety, without apparently any announcement in the older *Pyrotherium* deposits. This fact in itself would seem to indicate that they were migrants from another region, and while we are as yet unable to place these deposits in the time-scale with accuracy, it is yet highly probable that the Santa Cruz beds are not older than our North American Oligocene. In North America the Ganodonta appear in the very earliest Puerco deposits, and continue without interruption into the Bridger, where they disappear. No evidences of them have up to date been detected in the Uinta or White River beds.

"Now it is currently believed by geologists that no land connection existed between North and South America from the close of the Cretaceous to the close of the Miocene, when an extensive land bridge was formed. I am not familiar with the geological evidence upon which the conclusion rests, but if one is permitted to judge from the subjoined statements of Mr F. C. Nicholas, it is at the very least open to question. It is, of course, possible that the Ganodonta may have reached South America by way of Europe, Africa, and Antarctica, but on the whole it seems infinitely more probable that there was a land bridge of short duration during Eocene time between North and South America, and that they reached their destination in this way, than by the questionable and circuitous route just mentioned. If they gained entrance into South America by the European-African route, it seems indeed strange that they should have left no remains in the later Tertiaries of Europe. With the exception of a single specimen of *Calamodon Europaeus*, from deposits corresponding with the Wasatch in age, all traces of the American Edentata are absent in Europe, Asia and Africa."

To the first paragraph in this quotation no exception can be taken. With regard to the second, we have not the pleasure of being acquainted, either personally or by his writings, with Mr F. C. Nicholas, who may be a most excellent person, but the rambling extracts from a letter of his, which Dr Wortman prints in a footnote, can scarcely affect the problem of a land connection between the two Americas in early Tertiary times. Apart from this, the

evidence adduced by Dr W. B. Scott and others as to the separation of North and South America cannot be overthrown by the conclusions drawn from one group of animals, more especially when an alternative route of migration will explain the facts equally well, if not indeed better.

THE OLD WORLD EDENTATE MAMMALS

WHILE it may be admitted that one swallow does not make a summer, it cannot be contended that a single tooth is not amply sufficient to prove the existence of the group of animals in the country where it was found. And as Dr Wortman expressly states that *Calamodon europaeus*—founded on a canine from the Swiss siderolithes—is a member of the Ganodonta, there is ample evidence of the existence of that group in Europe during the Eocene. Probably Dr Wortman is unaware how rare mammalian fossils are in those deposits, and why he should make a point of their absence from the later European Tertiaries passes our comprehension. With regard to Africa, no Eocene or middle Tertiaries are known, and consequently no arguments can be drawn one way or another. Moreover, it is known that when the later South American ground-sloths succeeded in entering the northern half of the New World during the Pliocene, they flourished excellently well, and if their ancestors reached the South from the North, it is difficult to see why the group should have immediately died off in the latter area.

To our own thinking it is much more probable that the Eocene Ganodonts of the northern hemisphere migrated southwards from Europe to Africa, and eventually reached South America by that route, as appears to have been the case with certain other groups of mammals. This, of course, opens up the question whether the Old World, so-called Edentates may not after all really belong to that group. Without denying the possibility of this, it may be urged that whereas the skulls and limb bones of the Ganodonta are strikingly like those of the South American edentates, those of *Manis* and *Orycteropus* are as strikingly unlike. If, therefore, they belong to the same stock, they would appear to have diverged before the Ganodonta assumed their characteristic type. But as this was acquired in the early Eocene, the Edentate origin of *Orycteropus* and *Manis* seems very problematical. At the same time we have at present no other group in which to look for the parentage of those strange creatures.

NEW LIGHT ON THE OVA OF VERTEBRATA

In the series of observations published by K. Mitsukuri, of Tōkyō, in the *Journal of the College of Science* of the Imperial University,

Japan, we have a fresh instance of the admirable work done by certain of the Japanese morphologists. Mitsukuri's researches concern the fate of the blastopore, the relations of the primitive streak, and the formation of the posterior end of the embryo in *Chelonia*, together with some remarks on the nature of meroblastic ova in vertebrates. But, as is not infrequently the case, the most important results are those which receive least consideration in the title of the paper. The nature and fate of the 'yolk-plug' (or cell-mass projecting between the lips of the blastopore), which undergoes very complex changes and shiftings of position, is far more interesting than that of the blastopore itself, owing to the theoretical considerations which Mitsukuri's view of it involves. The previously-asserted homology of this cell-mass with the yolk-plug of the Amphibia, and with a similar structure observed by Van Beneden in Mammalia, is well-maintained. The necessity for a re-classification of vertebrate ova into 'primary' and 'secondary' types is clearly established, if the theory of the loss and acquisition of yolk in vertebrate eggs several times in the course of phyletic development be correct. The primitive plate and yolk-plug in *Chelonia* are shown to be rudiments of a large primary yolk-mass which existed in the early history of amniote eggs. The large yolk-mass seen in amniote eggs of the present day has been secondarily acquired, and the enclosure of this mass by the blastoderm is a coenogenetic process having nothing to do with gastrulation. On the other hand, the enclosure of the primary yolk-mass by the blastoderm is closely connected with gastrulation. Mammalian ova are supposed to have lost even the secondary yolk-mass. Any comparison, therefore, between the various classes of ova can only justly be made when these facts are given due weight, and they are likely to throw additional light on questions dealing with the primitive character or otherwise of various groups.

PRIMITIVE METHODS OF TREPPANNING

In *l'Anthropologie* (vol. viii., pt. ii., 1897) a most interesting account is given by Dr H. Malbot, assisted by Dr R. Verneau, on the Trepanning of the Skull by the Chaouïas of the Aurès Mountains, in the province of Constantine, Algeria. A preliminary account of these people and their country was given in the previous number of the same journal. It is a most curious fact that in a remote district in the above-named region, this people of mixed racial origin practise trepanning on an elaborate scale, and apparently maintain this practice as an heritage from ancient times. Trepanned skulls have been found in ancient cemeteries in Algeria, and prove the practice to be an old one in the region. The Chaouïas have established a great

name for success in this operation, which they conduct in a manner which is characterised by great boldness, combined with decidedly rough and ready methods. The surgical equipment is of the simplest description, the principal instruments being a kind of auger (*brima*), or centre-bit rather, and two kinds of very rude saws (*menchar*) of peculiar hooked shape and very short cutting edges. The text-book, there is but one, is a manuscript, a copy of which is possessed by each qualified trepanner. The *brima* is used for exploring, and holes are drilled into the bone of the skull, at first through the outer table only, for examination of the *diploe*; but, if necessary, the hole is extended through the inner table, exposing the *dura mater*. Large portions of the skull are, if it is deemed desirable, removed through the agency of the *brima*, several holes being drilled with it very close together, and when, after some weeks, necrosis has destroyed the narrow bridges of bone between the holes, the whole piece of bone round which the holes were drilled is detached with a lever and removed. The saw is used for grave cases, and the sawed grooves are sunk to the inner table, the remaining thickness of bone being scraped away with a hooked instrument. In other cases the grooves are less deep, and necrosis does the rest of the work, the final detachment of the bone being effected as before with a lever. Prayers and incantations always accompany the operation. They must be needed! Some stubborn cases demand the trial of every class of trepanning, and at successive sittings the operator puts them all in practice; "C'est une véritable orgie de trépanation!" The most peculiar part of the whole thing is that the patient as a rule recovers, this being due rather to the natural physical qualities of the Berber race, than to the skill of the operator. Recovery may, in fact, be said to be *in spite of* the surgeon. Dr Malbot was fortunate enough to obtain a skull showing all the methods practised, a most striking specimen of which he gives a figure. The skull is now preserved in the Museum of Natural History at Paris.

This paper should be read in connection with Dr Robert Munro's paper on "Prehistoric Trepanning and Cranial Amulets," which has been lately republished in his book on "Prehistoric Problems." This gives a good and well-illustrated general account of ancient trepanning, a special reference being made to cases belonging to Neolithic times. The use of fragments of skulls as amulets is also gone into in detail, and the fact made clear that trepanning was in some cases surgical, in others posthumous, following Broca's famous memoir of 1876. Dr Munro gives a sketch of the geographical distribution of this operation, and discourses on the methods employed in early times. It is a pity that so few details regarding the practice of trepanning amongst modern primitive peoples are

forthcoming, and it is greatly to be hoped that attention may be directed to this custom wherever it occurs, as the procedure observed amongst races in a low condition of culture may help to throw further light upon the archaeological aspect of the question.

SPEAR-THROWERS FROM NEW GUINEA

MR T. JENNINGS (*Proc. Linn. Soc., N.S.W.*, 1896, p. 793) has recently described in detail and figured two Papuan spear-throwers of bambu from New Guinea. These instruments have only comparatively recently been recognised as occurring in New Guinea, though numbers have now been received in the various European museums. The type is interesting for its form, which differs from that of the well-known hook-ended spear-throwers of Australia, and resembles rather that of the socket-ended examples from the Caroline and Pelew Islands, figured by Dr von Luschan. The addition of a wooden flange as a rest for the spear is peculiar to New Guinea, and the carving on these rests is often elaborate, and is varied individually, no two, probably, being quite similar. The original design in nearly all cases has apparently been some animal form grotesquely treated. The two examples described by Mr Jennings differ somewhat in detail from those figured by Dr von Luschan in his more elaborate paper on the subject, published in the *Bastian Festschrift*. Mr Jennings adds a few remarks upon the peculiar geographical distribution of these implements, but his account does not aim at being a complete one, and the distribution is pretty well known.

CYCADS

In our last number (p. 85) we referred to some recent work by a Japanese investigator which gave additional interest to an ancient and always interesting group of plants. The Cycads are the oldest family of seed-plants. They had reached and passed their maximum (in Triassic and Jurassic ages) before the appearance of the angiospermous type which is dominant at the present day. Their habit, a simple, short stem with a crown of leaves, recalls the tree-fern much more than our dicotyledonous forest-tree with its widely branching axis and small deciduous leaves. And the discovery, of which we gave a short account last month, was only an additional evidence of the fact, recognised now for more than thirty years, that Cycads, if not a connecting link, are at any rate representatives of a type of plant-life occupying a place in the scale of evolution between ferns and those seed-plants in which the ovules are packed away in a closed ovary-chamber. Their occurrence to-day is what we should expect in a disappearing but

once dominant group. There are only nine genera with about seventy-five species, but the order is widely distributed in the warmer parts of the earth, though individual genera and species have a very limited distribution. The old world has five genera, the new world four, but America possesses by far the greater number of species, Central America and Mexico being the richest areas, while Australia is the largest centre in the old world. *Cycas* (sixteen species) is the most widely-spread type, occurring in the warmer parts of Asia up to south Japan, in Australia, Polynesia and the Malagasy Islands. *Stangeria* and *Bowenia* are monotypic genera from Natal and Queensland respectively. *Dioon* has two species in Mexico; *Encephalartos*, twelve in South and tropical Africa; *Macrozamia*, fourteen in Australia. *Zamia* is the largest genus with thirty species, and is found from Peru to the West Indies and Florida; *Ceratozamia* is Mexican with six species, and *Microcycas* is a monotypic genus from Cuba. But a much larger number of fossil genera have been described, chiefly from leaves, though fruits and other reproductive organs are also known. Thus Engler in his *Pflanzenfamilien* enumerates twenty-three "more important" ones found almost exclusively in Europe, but occasionally in Greenland and Spitzbergen. Our nine genera are obviously scattered remnants of a once large and dominant family. Even individuals are isolated; except in the case of species of *Cycas* they are few and far between.

In the June number of the *Botanical Gazette*, H. J. Webber gives an account of his investigations into the structure and behaviour of the pollen-tube in a species of *Zamia*. One of his figures shows a peculiarity in the growth of the tube, which at first penetrates the nucellus for a short distance and then resumes growth at the other end, that, namely to which the grain is still attached. The important generative cell remains at the pollen-grain end in which it is carried down into the cavity above the archegonia or female organs. Webber describes two centrosome-like structures in this generative cell, the function of which is doubtful. The most interesting part of his communication is contained in a note which records the discovery, as the paper was going through the press, of motile antherozoids. As to how or where they arise, whether they are or are not in any way connected with the strange bodies in the generative cell, we are left completely in the dark, and can only hope for a continuation in our next.

THE FOSSILS OF THE ENGLISH CHALK ROCK

THANKS to Mr Henry Woods, we have now an intelligent and careful account of the mollusca of one zone of the Cretaceous

system of England. We say intelligent advisedly, because Mr Woods has not included in his lists those scraps of fossils which are considered by some authors worthy to occupy their text and their plates. The mollusca of the Chalk Rock have been described in the *Quarterly Journal of the Geological Society*, vols. lii. and liii., and comprise ten cephalopods, sixteen gasteropods, and twenty-nine lamellibranchs, and of these some seven of the first group occur in Saxony and Bohemia, two or three only of the second group, and about one half of the third. Fossils from this zone are rarely obtained in a perfect condition, and are frequently denuded of their shell, but Mr Woods has succeeded in figuring some typical specimens which will be useful to the collection. In looking through part ii. of his paper, we do not see any mention of Dover, where the Chalk Rock is easily accessible and fairly rich in fossils; indeed, in a few hours we have collected all but two of the gasteropods mentioned by Mr Hill in the *Quarterly Journal*, vol. xlii. As the energetic members of the Geologists' Association were observed in numbers at the Chalk Rock of Dover last Easter, Mr Woods might easily have obtained a list of specimens. It is gratifying to read that the author intends to proceed with the *Inocerami*, for they are in worse confusion than most other shells. His synonymies of *Lima hoperi* and *Spondylus spinosus* are interesting and important. Mr Woods will forgive us perhaps if we point out to him that *Salvius* did not print the molluscan part of Linné's edition xii. until 1767, and therefore the date cannot be 1766; but why not use the tenth edition, 1758, now almost universally recognized?

A list of other remains identified is supplied, and discussions on the distribution and relations of the fauna and conditions under which the Chalk Rock was deposited are given. The whole is a useful and valuable paper which will be largely in request.

TIERRA DEL FUEGO

IN September last year we were favoured by Dr Ohlin with an account of the zoological results of Baron Oscar Dickson's expedition to Tierra del Fuego. A preliminary notice of the geographical results of that expedition is now published in the *Scottish Geographical Magazine* for August. The country consists of a woodless tableland in the north, and a mountainous district in the south, the latter being the extreme continuation of the Cordilleras. The boundary between the two zones is almost a straight line. The northern country is stated to be of Tertiary age, covered partly by gravel and partly by moraine.

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JOHANNES JAPETUS SMITH STEENSTRUP

Born March 8, 1818; died June 20, 1897

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I

Steenstrup

JOHANNES JAPETUS SMITH STEENSTRUP was born on March 8, 1813, in the northern part of Jutland, in the district termed Thy, where his father was a parson. In the year 1832 he was sent from the cathedral-school of his native province at Aalborg to be a student at the University of Copenhagen. In two succeeding years (1833-35) he was obliged to remain in the paternal home, occupied with teaching his younger brothers and with natural history excursions into his native country, collecting numerous examples of its interesting natural productions, its plants and animals, its fossils and geological features. Of scientific facilities or aids he had very little; a copy of the published parts of the celebrated "Flora Danica," of Linné's "Systema Naturae," of O. M. Müller's "Prodromus zoologiae danicae," were, I believe, almost the only books of science available for Steenstrup in these early times of his scientific self-training and self-education; his only helper at this time being a gifted parson, his uncle, formerly a pupil, especially in botany and entomology, of the renowned naturalist and teacher Melchior, at the college of Herlufsholm. After his return to the university in 1835, in the full bloom of a self-made young naturalist, he became the pupil and friend of Schouw, the botanist, of Forchhammer, the geologist, and of Reinhardt, sen., the zoologist, whose ingenious lectures left an impression on Steenstrup's mind never to be effaced. Among the particular friends of those days of his youth were the gifted botanist Drejer, lost at an early age, Liebmann, Schouw's successor as Professor of Botany after his return from Mexico, Reinhardt, jun., the celebrated zoologist and traveller in Brazil, etc. Only two years after his return to the university Steenstrup earned the honours for two prize essays, the one (never published, only epitomised in my text-book, "Dyreriget," and therefrom in Palmen's work on the migrations of birds), "on the differences between the wanderings of birds and fishes," the other, published afterwards (1842) in the *Transactions* of the Danish Academy of Science, "on the geological investigation of certain forest-moors on Seeland," etc.—a work of great sagacity and acute observation, the first to elucidate the sequence of the different forest-vegetations charac-

teristic of the early periods in the recovery of our country after its emergence from the waves and the close of what is now termed "the Ice-Age"—the ages of the aspen (*Populus tremula*), of the fir (*Pinus sylvestris*), the oak (*Quercus sessiliflora*), and ultimately the alder (*Alnus glutinosa*) and the beech (*Fagus sylvatica*). Much time was destined to elapse before these studies of the gifted rising naturalist, so important for the history of the life of our globe, were taken up elsewhere in other northern regions.

In the year 1839-40 Steenstrup was sent by the Government, with an Icelandic student, Hallgrimson, and Mr Schytte, afterwards Professor of Chemistry in Chile and Governor at Puntas Arenas on the Straits of Magellan, to Iceland for an economic investigation of this country. The story of this voyage, though interesting to those who have had the good fortune to hear Steenstrup's reminiscences of the country, its nature and its people, was never published; nor was his interesting and fruitful discovery that the so-called "Surturbrand" in the Tertiary trap formation of Iceland contained a series of remains of an arboreal vegetation, with its tulip-trees (*Liriodendron*), etc., most resembling that of certain subtropical regions, until his Icelandic materials and figures were placed in the hands of Oswald Heer. Steenstrup's investigations on the volcanic formation of Iceland have been taken up by younger minds, who have no doubt been much benefited by the information that was in Steenstrup's possession. Another discovery made by Steenstrup on this trip to Iceland happened during some sunny days near the coast of Norway, viz., the discovery of the metamorphosis of crabs (*Hyas araneus*) and soldier crabs (*Pagurus bernhardus*), at a period when metamorphosis in Crustacea was very little known, and therefore was received by excellent zoologists with considerable doubt. Steenstrup's letters to Reinhardt on this subject were printed in the *Proceedings* of the Royal Danish Academy in 1870. The specimens collected were also sent down to his teacher. They were seen here by Rathke, who described them together with the material collected by himself. The history of the metamorphosis in the higher Crustacea now occupies an extensive literature; among the first pioneers in this important chapter Steenstrup's name must have its place.

Shortly after his return from Iceland in 1841 Steenstrup was appointed lecturer in Botany and Mineralogy (geology) in the Academy of Soroe in Seeland, the only place in Denmark where Natural History and a knowledge of modern languages had its rightful place among the classical lines of study. Here he remained until 1846, occupied especially with two of his best known works, published as programmes of the college, viz., "The Alternations of

Generations" (1842), and "On the Existence of Hermaphroditism in Nature" (1845). The first of these is too well known to need any explanation here; it can only be wondered that this doctrine so wonderful at the time of its publication has been so little modified in its essential points and lost or gained so little in extension since that time. Space forbids me to enlarge further on this topic, which more than any other of Steenstrup's writings has spread his name and fame over the whole civilised world. His second work, that "On Hermaphroditism," was less successful, though its subject was in intimate connection with lines of thought resulting from or connected with "metagenesis," as it is now generally termed. One may admire the author's acuteness of perception and the extent of his comparative studies, and confess that he quite rightly abolished many cases of unfounded hermaphroditism among inferior animals; but it must be allowed that hermaphroditism is still fully recognised, with few exceptions, among leeches, flukes, tapeworms, pulmonate and opisthobranch snails, barnacles, etc. (Tardigrada have lately been thrown off), without our being able to give an adequate natural reason for its presence in some tribes and its absence in others. In recent times Steenstrup's memoir has awakened the important remark, that in all probability hermaphroditism has not been the primordial rule in any division of higher or lower zoological rank, but must be a "later acquisition" in the course of evolution, for which no satisfactory reason can yet be given. While speaking still of Steenstrup's residence at Soroe, it should be mentioned that here he pursued, in the course of other faunistic studies, an examination into the specific duplicity of our common frogs (*Rana temporaria*) and the distinctive characters of what he termed *R. oxyrhinus* and *R. platyrhinus*, which have played a rather important part in the recent study of the Anourous Batrachia.

In 1846, after the death of Reinhardt, sen., Steenstrup was nominated to replace him in the chair of Zoology at the University of Copenhagen, and as Director of its modest zoological collection. He was a beloved and admired teacher for the students of medicine and for the pupils of the polytechnic school, and at the same time the gifted chief leader of the "Natural History Society." A member of the Royal Society of Science from 1842, he was its secretary after Forchammer's death until after years of great activity he gave up this post in 1878. The election to the Presidency after Madvig's death he declined, as he had more than once declined the Rectorship of the University, being anxious not to be drawn too much away from his scientific studies and his professorial duties. In 1848 he was with Forchammer placed at the head of the "Royal Natural History Museum," with the recommendation of the Minister of Education to promote its union with the University Museums—a

proposal which naturally enough interested Steenstrup much, but met with some opposition, not only from the University, which reasonably feared the increased pecuniary obligations involved in such a scheme, but also from the majority of the keepers at the Royal Museum. At last the battle was won by the bill of 1863, which ordered the construction of a much wanted building in the grounds of the University. It was finished and opened to the public and to Science in 1870, and has since been the handsome, but perhaps not sufficiently large home of zoological science with us, constructed by the gifted architect, Chr. Hansen, whose genius was, I believe, strongly fertilised by Steenstrup's ideas. Steenstrup was not, as originally planned, the sole director of the new museum, but by the election of the University the president of its council, consisting of two keepers (inspectors), Schödte and Reinhardt, and himself as administrators of its different departments. I shall not here speak of the difficulties and painful controversies connected with this organisation. Steenstrup retired from his position as Professor of Zoology in the year 1885, after a painful period, rendered more distressing through an unfortunate accident (a fracture of collum femoris). I shall confine myself to a short resumé of his chief scientific work from 1846 to 1885, the years of his professorship.

It was one of Steenstrup's characteristic features that he was not only an excellent zoologist and a specialist in some of its branches, but also a good geologist and botanist, capable of discussing many topics relating to different sciences; and it may be said, that he had a certain predilection for those points of science, where its different sections meet and intercross. It will therefore be easily understood that a man with his abilities and constitution of mind must play an important part in a large scientific community. It is, of course, a difficult task to classify his works, which can often be referred to more than one of the related sciences, and whose value may be judged from different points of view. It will be understood that while his humble successor in the chair of Zoology since 1885 may think himself entitled to judge of his purely zoological work, he must speak somewhat more discreetly, notwithstanding the partly natural historical character of Steenstrup's archaeological and related publications, on this part of his literary work, and leave the ultimate judgment to his historical and archaeological colleagues.

One of Steenstrup's great services was, that he induced—what was then a rarity—some of our excellent seafaring men of the navy or of the merchant line, to devote their leisure hours to collecting the animals of the seas through which they sailed, making careful notes of the localities examined with their nets, and in this manner

furnishing the museum with pelagic and other material from almost every part of the ocean. Several parts of this "Plankton" have since been worked upon by his pupils and others (e.g., Boas on the Pteropoda, Traustedt on the Salpae, Lütken on the Dolphins and the "hemi-metamorphoses of fishes," Bovallius on the Hyperidae, etc.). With this series of studies may also be reckoned the memoir of Steenstrup and myself on the parasitic Entomostraca of the ocean with several other forms of the same tribe; also the former's anti-critical note on the genera *Silenium*, *Lesteira* and *Pegasimallus*, and his papers (too numerous to be enumerated here) on Cephalopoda (*Notae teuthologicae*, etc.) in the *Transactions and Proceedings* of the Academy of Science, in the *Videnskabelige Meddelelser fra den naturhistoriske Forening*, and elsewhere in popular journals. I shall dwell, however, more particularly only on two points. Firstly, there is his surprising demonstration that the apparently abnormal development of one or occasionally two arms in male cuttlefishes, hitherto overlooked or not understood, was in fact the homologue of the well-known "hectocotyle" in the pelagic Octopoda. His eager desire to throw the light of his genius and of his science on obscure problems, led him also to investigate the tale of the wonderful sea-monk, the monster that was cast ashore in our vicinity in the sixteenth century, described and figured by Belon, Rondelet and Gesner, and playing an important part in the semi-mystic Natural History of the Renaissance. Nobody had been able to decipher this enigmatical monster until Steenstrup deprived it of its fabulous investment, demonstrated it to be simply a decapod giant cuttlefish. Specimens of this same kind (*Architeuthus*) have been thrown on our shore, formerly and later on the shores of Iceland, Faroe, Jutland, Newfoundland and Japan, and happily one of our captains did find such an animal floating in the Atlantic, and secured to Steenstrup some of its most important parts. Steenstrup's full account of these remains was partly in print, though never completely published; but some of his plates have been placed in the hands of his fellow-zoologists. To the other purely zoological articles of Steenstrup, I shall only allude briefly, namely, to those on *Sphenopus* (*Sabella marsupialis* Gm.), on *Philichthys*, *Rhizochilus*, *Xenobalanus*, *Pachybdella* and *Peltogaster*, on the enigmatical objects correctly interpreted as the "gillrakers" of *Selachus maximus*, on the natural systematic place of the walrus, etc. His interpretation of the wandering of the eye in young flounders has not been accepted with unanimity, but still has some trustworthy points to fall back upon. Our common memoir on the *Mola*-tribe (*Orthogoriscus*) and its larval stages, has not been published in its complete form; perhaps it may be so still. Several palaeontological papers on mammals, birds and reptiles (turtles) found in our

peat-beds or other formations have appeared at least in abstract, as also some account of the refuse-heaps or shell-heaps ("kitchen-middens") of our shores, whose correct interpretation was the work of Steenstrup, and has obtained world-wide notice and given birth to many investigations in other countries. Some papers on Helmintha (*e.g.*, *Fasciola intestinalis*) should not be forgotten. His interpretation of the Brachiopoda as not belonging to the true Acephala has in later time become popular; his interpretation of the partly operculated "Palaeozoic corals" (Cyathophyllidae) as not being Anthozoa-proper, but something else, perhaps allied to Serpolidae, Hippuritidae or Brachiopoda, has met with decided doubt and opposition, but in my opinion, not found its final decision. In the study of the newer (Glacial) geological formations, Steenstrup took an active part with his lamented younger friend and colleague, Johnstrup, and some of his later papers briefly give his views on some of the theories advanced in Scandinavia concerning this important part of Scandinavian geology. That he with Nathorst, the Swedish botanist-geologist, partook in the discovery of the earliest glacial plant remains in our Scandinavian peat-bogs, should also be remembered. He was present at most of the meetings of the Scandinavian naturalists during the period of his scientific activity and has left the marks of his influence in the reports of the meetings of the Americanists and Archaeologists at Copenhagen and Brussels. Beyond his travels in Iceland and his visit to the Faroe Islands, he made several journeys to Germany, France, Northern Italy, Switzerland, Dalmatia, and England, enriching his knowledge and adding to his acquaintance and friendly relation with eminent men of many countries and many sciences. His scientific correspondence would fill many volumes.

Already in the earlier part of his career, Steenstrup had published some papers of a chiefly historical aim, throwing light on obscure phenomena, elucidated from a naturalist's point of view (*e.g.*, on the so-called "havgaerdinger," on Ottar's relation to King Alfred on his travels in northern seas, and on the passage of King Harold through the Limfjord). After he had retired from his zoological professorship in 1885, he treated with great emphasis several archaeological problems of the same character (*e.g.*, the Haellristningar, the voyages of the Zeni, the Yak-Lungta-Brakteats, the silverplates found at Gundestrup, the mammoth station at Predmost in Bohemia. Most of them have been published, though not the first. I shall not do more than point out the existence and interest of these remarkable papers, not being competent to pass a scientific judgment upon them; but, at the same time, I would express my conviction, that they will remain through all future time a testimony of the great insight, sagacity and knowledge of my gifted

friend and teacher, the glory of his country, of Europe, and of his century. That he was honoured with the highest distinctions from his country and from many other sources, I need not tell, nor enumerate the learned societies (Stockholm, Christiania, Berlin, Paris, London, etc.) of which he was a member. This sketch of Steenstrup's life and work may appear longer than usual to the readers of this journal, but it is not long or detailed enough to do justice to what ought to have been said. I cannot conclude without naming his wife, Ida (*née* Kaarsberg), the love of his youth, lost several years before her husband died. Several children died earlier or later; one daughter is left, and there is one son, Johannes Steenstrup, Doctor in Law and Professor Rostgaardianus in History at our University, whose work on the history of the Normans will especially be known to many English readers.

CHR. FR. LÜTKEN.

II

Does Natural Selection play any part in the Origin of Species among Plants?

INTRODUCTION.—The objects of the present paper are to answer this question in the negative, and to prove that natural selection is a superfluous factor as an aid in the origination of new varietal characters; though it has much to do with the "survival of the fittest" in "the struggle for existence" among beings in any particular locality. It is, of course, the Darwinian conception that these factors are somehow concerned in the origin of species; but I would maintain that they must be kept totally distinct from it. Darwin, in truth, insisted upon this fact himself; that whatever the causes or origins of variations might be, such were questions with which natural selection had nothing whatever to do. His words are:—"The direct action of the conditions of life . . . is a totally distinct consideration from the effects of natural selection . . . [it] has no relation whatever to the primary cause of any modification of structure."¹ What I wish to show is that sufficient variations to constitute a variety are always the result of a direct or indirect response to the "definite action" of a new environment; indeed many, if not all the organisms, of whatever kind they may be, which are subjected to it, often vary more or less in a like manner.² It will then be seen at once that not only are there no "indefinite variations" for natural selection to deal with, but as a consequence its *raison d'être*, as an aid in the origin of species is gone; and it can take no part in the origination of varieties.

I wish also to point out that Darwin's theory of natural selection rests entirely upon a series of *à priori* assumptions or deductions, which have never been verified; nor, indeed, do they seem capable of verification.

Definition of a Species.—In order to be clear, it is desirable to state precisely what one understands by the term "Species." According to the method pursued by systematic botanists in describing plants, a species may be defined as follows:—"Any particular species of a genus is known by a collection of characters taken from any or all parts of the plant. These characters are, or

¹ "Animals and Plants under Domestication," vol. ii., p. 272.

² Hence arises the *facies* characteristic of aquatic, desert, alpine, and other plants: as I have described in my work—"The Origin of Plant Structures."

are theoretically assumed to be, constant." One or more of these characters may be found on another species, which in a similar manner is known by its collection of constant characters.

What may have been their origin, and how the survival and maintenance of any superficial characters of a plant have been secured, are philosophical questions with which the systematist has no concern at all.

Useless Characters.—Before showing that the hypothesis of natural selection is superfluous in the origination of varietal characters, let us turn to the descriptions of plants given in some standard work, say, Sir J. D. Hooker's "Students' Flora of the British Isles." It will be found that many characters are taken as specific or generic which cannot, with any show of reason, be regarded as specifically useful; such as the numerical excess or deficiency in the number of parts in the floral whorls; *e.g.*, *Gentiana campestris* is described as having the calyx "four-partite"; while in *G. amarella*, it is "five-lobed"; but fours, fives and sixes may be often found on one and the same plant, as in a corymb of elder flowers, due to an accidental deficiency or excess of nutriment, respectively; and no vital importance can be attributed to the trivial specific distinction between "partite" and "lobed." Such illustrations of quite unimportant characters regarded as specific can be multiplied to any extent; but they are some of the very characters which Darwin admits are not due to natural selection. He says:—"We thus see that with plants many morphological changes may be attributed to the laws of growth and interaction of parts, independently of natural selection."¹ They are, in fact, simply the inevitable results of a response to environmental conditions, using the term in the broadest sense.

With regard to such indifferent characters being hereditary, Darwin first says that he "felt great difficulty in understanding the origin or formation of parts of little importance; almost as great, though of a different kind, as in the case of the most perfect and complex organs,"² and he devotes a section to a theoretical interpretation of them. Indeed he, on several occasions, recognises the existence of useless characters; *e.g.*, he says, "I am inclined to suspect that we see, at least in some of the polymorphic genera, variations which are of no service or disservice to the species; and, consequently, have not been seized on and rendered definite by natural selection."³ In this passage the word "disservice" almost seems as if he had a suspicion that "injurious" characters might sometimes be present, though he elsewhere says:—"Any actually injurious deviations in their structure would, of course, have been

¹ "Origin of Species," 6th ed., p. 175; see also p. 367.

² "Origin, etc.," p. 156.

³ "Origin, etc.," p. 35.

checked by natural selection." Secondly, the following are Darwin's words with reference to the inheritance of characters which are no longer useful:—"No doubt the definite action of changed conditions . . . have all produced an effect, probably a great effect, independently of any advantage thus gained. . . . I fully admit that many structures are now of no direct use to their possessors, and may never have been of any use to their progenitors." . . . He mentions the webbed feet of upland geese, etc. . . . "With these important exceptions, we may conclude that the structure of every living creature either now is, or was formerly, of some direct or indirect use to its possessor,"¹ He would thus include all rudimentary organs as having been formerly useful, but now useless; of these he remarks that rudimentary organs from being useless are not regulated by natural selection, and hence are variable. If they be so in the animal kingdom, they are not so in the vegetable; *e.g.*, the staminodes and rudiments of ovaries of flowers are constant in form to each species, genus or order which is characterised by them, respectively; as, *e.g.*, *Erodium*, *Samolus*, *Mercurialis*, *Parietaria*, *Valerianeae*, *Myrsineae*, etc., and are recognised as permanent diagnostic characters.

Injurious Characters.—In many flowers there have been acquired and retained by heredity, what may be called by Darwin's term "disservice," or even "injurious" characters. For if, *e.g.*, the use of flowers be to set good seed, then anything which tends to hinder that process is obviously injurious. Such occurs in the structure of the flowers of most orchids, and in many adaptations to insect fertilisation, as dichogamy, protandry, polymorphism, etc., whenever they tend to bar self-fertilisation.

For it need hardly be observed now, that Darwin's assumption from the numerous adaptations in flowers for intercrossing by insects, that self-fertilisation was "injurious," was based on a quite erroneous deduction altogether. The fact being that in nature autogamous, or self-fertilised plants, are by far the most prolific, perfectly healthy, most abundant in individuals, and most widely dispersed.²

On the other hand, all special adaptations to secure self-fertilisation are obviously useful, are quite as numerous and excellent in the adjustment of the organs, as are those for intercrossing.³

Now it is worth while observing that the result of such injurious

¹ "Origin, etc.," p. 160.

² The reader is referred (should he require it) to the writer's papers on "Self-fertilisation," *Trans. Linn. Soc.*, 1877; Review of Darwin's "Cross and Self-fertilisation of Flowers" in "Gardener's Chronicle" (1877); and "The Origin of Plant Structures."

³ See Kerner & Oliver's "Natural History of Plants;" "Autogamy," vol. ii., p. 331, ff.

features may even be the actual extinction of a species; for it is conceivable that if a plant cannot set seed by self-fertilisation, and is not crossed by insects or the wind, it will die out, if it be an annual or not propagated by its vegetative system. It will thus be eliminated by natural selection.¹ But the process falls within the subject of the distribution of species, both in time and space, and has nothing whatever to do with the origination of such harmful structures; which, as long as they exist, are regarded as specific or generic characters.

The survival of the fittest, therefore, and the destruction of the least fit and incapable to survive, are questions altogether independent of the Origination of Structural Variations, upon which the survival, or destruction in some cases, may actually depend. The reader must constantly bear in mind Darwin's words which I again quote, because of their importance: "The direct action of the conditions of life . . . is a totally distinct consideration from the effects of natural selection . . . [it] has no relation whatever to the primary cause of any modification of structure."² This last is the sole matter with which I am concerned.

Individual Differences.—These according to Darwin³ and Dr Wallace are the chief materials for natural selection to act upon. As I have already fully discussed this subject in *Natural Science*⁴ and pointed out that as a rule they are quite incapable of giving rise to varietal characters which a systematist would take note of, I need say no more than invite the reader's careful attention to my article.

I might, however, briefly point out a fallacy in Dr Wallace's conclusion. He has given numerous tables in his work, "Darwinism," and argues that any excess in dimension of an organ from the mean is eliminated by natural selection; so that a species keeps its dimensions pretty constant, annually.⁵ But no intimation is given as to how great a deviation, in excess or deficiency of the mean, is required to prove destructive to the creature itself. Experience, however, shows that both nanism and gigantism are common phenomena in nature among plants; in which the customary deviations are vastly exceeded. Moreover they can be induced to arise under cultivation coupled with perfect health, fertility and heredity. Therefore, the whole of this argument falls to the ground.

Supposed Requirements of Natural Selection in the Formation of New Varieties.—The primary condition assumed by Darwin and Dr Wallace is a large population. In order to produce a new variety these writers tell us that "in the great majority of cases a new species arises amidst the population of an existing species."

¹ "Origin, etc.," p. 57.

² "Animals and Plants under Domestication," vol. ii., p. 272.

³ "Origin, etc.," p. 34.

⁴ Vol. vi., p. 385 (1895).

⁵ *Journ. Linn. Soc. (Zool.)*, vol. xxv., p. 483.

"The greatest danger," writes Dr Wallace, "to a species under new and adverse conditions is, that it should not be able to adapt itself to them with sufficient rapidity. It is for this reason that, as Darwin concludes, new species arise from those which have a large population, which occupy a wide area, and which present much variation, a combination . . . rarely found except in continental areas."¹ How far is this hypothesis borne out by facts? As a matter of fact the majority of species of a country have not a large population, nevertheless many of such species have varied as much as, if not more, indeed, than the more gregarious species with large populations; thus, it is easy to think of plants, of which large populations exist, generally gregarious, and therefore supplying the primary condition supposed to be requisite for natural selection; but the remarkable feature about them is that they have never been known to vary! Thus, Sir J. D. Hooker gives no varieties whatever to any of the following species, *Ranunculus ficaria*, *Caltha palustris*, *Lychnis diurna*, *Erica cinerea*, *Bellis perennis*, *Urtica dioica*, *Galium verum*, *Scilla nutans*, *Lemna minor*, *Pteris aquilina*, &c. And when we cross the channel (though England is really or physically part of the Continent) we find no more signs of variation there, whether in France, Germany, Switzerland, the Tyrol, &c.

On the other hand, take an extremely common plant, *Polygonum aviculare*; though abundant, it is scarcely a social plant, at least, to the extent of those mentioned. It produces several varieties, but are they found in the midst of the commonest, say, the roadside type? Sir J. D. Hooker says:—"Var. *P. littorale* (littoral) a passage to *P. maritimum* (maritime); Var. *agrestinum* (field form); Var. *arenastrum* (sand-loving form); Var. *rurivagum* (wayside form);" hence these varieties are not found in the midst of the commonest form, but away from it, in localities characterised by special physical features. In other words, these varieties arise by self-adaptation to their special environments, respectively.

The second condition requisite for variations consists of "changed conditions of life." Both Darwin and Dr Wallace admit that "a change of climate and food" is requisite for a new variety to arise among the parent type. The latter writer says:—"Now let some important change occur, either in climate, in abundance of food, or by the irruption of some new and hitherto unknown enemies, a change which at first injuriously affects the species."² Similarly Darwin writes:—"Let the external conditions of the country alter," and again, "take the case of a country undergoing some . . . change."³

The question at once arises, where and when do we find these changes occurring in, or coming to, any particular district, where some species with a large population happens to be? Is nature

¹ *Loc. cit.*, p. 484.² *Loc. cit.*, p. 483.³ "Origin, etc.," p. 63.

dependent upon geological catastrophes for producing variations in plants and animals? Indeed, this would seem to be Darwin's view in his discussion on geologic time, in which he says:—"It is probable, as Sir W. Thomson insists, that the world at a very early period was subjected to more rapid and violent changes in its physical conditions than those now occurring; and such changes would have tended to induce changes at a corresponding rate in the organisms which then existed."¹

But when we find that one species will change into another recognised species under our very eyes, if its environment be altered, why need one appeal to millions of years for aid? Dr Wallace, *e.g.*, notes how "*Arabis anchoretica* has tissue-papery leaves due to its growth in hollows in the rock. Seeds of this plant when cultivated at Kew produced the common species *A. alpina*. The same thing occurs with many plants as every cultivator knows."²

Darwin and Dr Wallace agree in requiring "rapid adaptation," but Darwin admits "that natural selection generally acts with extreme slowness."³

Now, if nature has to wait for catastrophes before some "changed conditions of life" come to her organisms, is not this something like trying to bring the mountain to Mahomet, instead of letting Mahomet walk to the mountain? Which is easier to do, to let plants and animals migrate to a place with a different climate and abundance or deficiency of altered food, rather than imagine the latter to come to them?

Migration is so obvious a process that Darwin cannot help alluding to it, as when he says:—"Among animals which unite for each birth and are highly locomotive, doubtful forms ranked by one zoologist as a species and by another as a variety, can rarely be found within the same country, but are common in separated areas."⁴ They have not, therefore, arisen at one common spot.

A new climate and abundance of food are often supplied by domestication and cultivation, and the anticipated results follow, *viz.*, variation *ad libitum*, the consequences also being often hereditary as they are in nature.

Acquired Characters are Hereditary in Plants.—Dr Wallace writes:—"Climate and Food undoubtedly produce modifications in the individual, but it has not yet been proved that the modifications are hereditary. If this could be proved the whole discussion on the heredity of acquired characters would be settled in the affirmative."⁵ But surely cultivation proves it every day? Our garden vegetables are all derived from wild plants, and they come true by seed.

¹ "Origin, etc.," p. 286.

² *Natural Science*, vol. v. p. 182.

³ "Origin, etc.," p. 84.

⁴ "Origin, etc.," p. 37.

⁵ "Darwinism," p. 489.

What more do we want to prove that acquired characters are hereditary? I do not understand what he means when he says:—"In every case these changes can be interpreted as . . . adaptations or individual, non-hereditary modifications in the case of plants."¹ That garden races are adaptations to their environment is obvious, and to say that they cannot be hereditary is, as it seems to me, to shut one's eyes gratuitously to the most conspicuous facts. The "Student" Parsnip was "fixed" in five years, *i.e.*, from 1847 to 1852, having been raised by Professor J. Buckman from seed of the wild plant, and it is still pronounced to be "the best in the trade"; its acquired characters have been, therefore, relatively fixed for half a century, though the plant's variability may never cease to exist, because no so-called "fixed race" is absolutely stable. Hence we constantly hear of Mr A's improved race of Mr B's pea, bean, or what not. Nevertheless, that the typical garden form is always reproduced, and that its sub-varieties or races come relatively true by seed, is all that is wanted to establish the truth of acquired characters being hereditary in plants.

Migration, essential.—With regard to the origin and fixation of varieties in nature a closer observation shows that, as a rule, contrary to the Darwinian view, new varieties of plants have not arisen among the parent types, but away from them. Thus, Sir J. D. Hooker, who in his knowledge of the geographical distribution of plants is *facile princeps*, says:—"As a general rule the best marked varieties occur on the confines of the geographical area which a species inhabits."² Darwin also quotes A. de Candolle's opinion that "plants which have very wide ranges generally present varieties; and this might have been expected (he writes), as they are exposed to diverse physical conditions."³ Precisely so; but then this is due to migration together with adaptation to the new physical environments; for the "diverse physical conditions" do not come to the plants where the large populations have been supposed to grow. It is interesting to see that both Darwin and Dr Wallace, after asserting the importance of large populations among which new varieties are said to arise, are compelled by facts to admit precisely the contrary. Thus, both Dr Wallace and Darwin observe that the struggle for existence will be "most severe between individuals of the same species; for they frequent the same districts, require the same food, and are exposed to the same dangers." Such is the condition said to be required for natural selection; but now, on the contrary, he tells us, "as an effect of this principle [?] we seldom find closely allied species of animals or plants living together, but often in distinct though adjacent districts where the conditions

¹ *Loc. cit.*, p. 490.

³ "Origin, etc.," p. 43.

² "Introductory Essay to the Flora of Tasmania," p. v.

of life are somewhat different." If so, and this statement is quite in accordance with Sir J. D. Hooker's view already quoted, how could the varieties have arisen in the midst of the plant type? Similarly, Darwin says that mountain breeds always differ from lowland breeds; and "a mountainous country would probably affect the hind limbs from exercising them more, and possibly even the form of the pelvis," &c. What is all this but the formation of new varietal structures by a response to the direct or definite action of the environment? But, then, it is obvious from Darwin's remarks that the mountain breeds are not supposed to have arisen among the lowland forms or *vice versa*; just as the submerged forms of *Ranunculus* could not have arisen among land buttercups or *vice versa*. Consequently Darwin could not shut his eyes to the fact that "isolation is an important element in the modification of species."¹ Again, he says:—"Migration and isolation are necessary elements for the formation of new species."²

On the other hand, Dr Wallace says:—"Physical isolation, I believe with Darwin [?], to be of comparatively little importance, and to have very rarely been the chief agent in modification."

If migration and isolation, which are only to be secured on the confines of the geographical area of a species, as Sir J. D. Hooker says, are so important, then it becomes obvious that the centre of the parent population is not the place, as a rule, to look for the origin of a new variety, but as far away from it as possible. From this it follows that the less struggle for existence there be with the parent type, the better it is for the origination of new varieties; and it is best of all where there is no struggle at all.

Dr Wallace enquired of two experienced British botanists if there "are any cases of well-marked varieties, which occupy a considerable area to the exclusion of the parent species, and do not occupy any area, or only a very small one with the type."³ One example of a *Rubus* was given him; but a more important question, however, as it seems to me, would be:—Is a sub-species or variety usually found within the area occupied by a large number of the parent type? Take, *e.g.*, *Hieracium*, a most variable genus; of this Sir J. D. Hooker writes:—"Variable as the genus is, the sequence of its forms is so natural as to have been recognised by all botanists. This sequence represents to a considerable extent the spread of the forms in altitude and area in the British Isles."⁴ Now *Hieracium* is not a genus with gregarious species; for though the sub-species and varieties are very many, the relative quantity of each is not particularly great anywhere; and thus, so far from lending any

¹ "Origin, etc.," p. 81.

² *Loc. cit.*, p. 494.

³ "Origin, etc.," p. 82.

⁴ 'Students' Flora,' p. 232.

countenance to Darwin's idea, that a species must have a numerous population to produce varieties, the rule seems rather to be that these two features do not necessarily coincide at all.

Supposed Aids to Natural Selection.—In order to secure the survival of the fittest, *i.e.*, a new variety among the parent form, it was perceived that two additional and hypothetical aids were necessary, *viz.*, (1) some degree of infertility with the parent, and (2) a rapidity of adaptation.

With regard to the first, all experience goes to prove that it does not exist; for when cultivators wish to fix a new race, as of cabbage, &c., they are obliged to grow it as far as possible away from the parent stock. Indeed, considering how freely species can be hybridised, the probability of an offspring refusing to be crossed by the same species is very small or nil. Neither Darwin nor Dr Wallace bring forward any examples of infertility with the parent among plants.

Secondly, a rapidity of adaptation is claimed hypothetically. This does often really exist, but it is a little uncertain whether these authors were aware of it. For when a plant finds itself in a new and markedly different environment, which strongly affects it, it then grows by self-adaptation in response to the new external influences: as when passing from water to land, or *vice versa*; from the wild state to the artificial soil of a garden; from lowlands to alpine or subarctic localities, &c., as I have shown in "The Origin of Plant Structures."

The Persistence or non-retention of new varietal characters.—To come to what Dr Wallace regarded as the most important point in his paper. Four times does he mention it, only slightly altering the expression, *e.g.*, he says:—"No attempt has been made to show, even hypothetically, how, through the action of known causes, such characters [useless ones], when they do arise, can become first extended to every individual of a species, and then be totally obliterated as regards any portion of the species which may become modified so as to constitute a new species. Useful characters thus strictly limited are the necessary and logical results of modification through survival of the fittest. No agency has been shown to exist capable of producing useless characters similarly limited."¹ As illustrations to meet Dr Wallace's demand, it may be observed that the races of cultivated pears are spineless; yet they are derived from the wild *Pyrus communis*, which has useless abortive branches as spines. Similarly is it the case with some varieties of plums derived from *Prunus communis*.

With regard to the retention of injurious characters, the

¹ *Loc. cit.*, p. 491.

obstruction to self-fertilisation produced by the rostellum is common in orchids, and generally occurs in all the species of any particular genus. Yet it is obliterated in *Phajus blumei*, *Chysis aurea*, species of *Chrysoglossum*, *Arundinia speciosa*, and *Eria flavesceus*, &c., so that these species set plenty of good seed by self-fertilisation, whereas 40,000 blossoms of *Dendrobium speciosum* set one pod. I have already had occasion to allude to the rudimentary organs of *Mercurialis*, *Erodium*, &c., which are retained in all the species alike.

Is Dr Wallace, therefore, justified in making the above assertion at all, or in demanding that either useful or useless characters should be limited? Why should either one or the other be obliterated when a new variation arises? The fact that a genus, which is the result of sufficient variation in a species (unless it be monotypic), does retain both useful and useless characters in some, many, or all of its species, shows that there are no grounds for his statement. Natural selection may demand it, but nature utterly refuses to be obedient to that theory.

Too great stress is laid upon a necessary fixity, as a proof of specific characters, by many writers. This is purely a relative matter. Cultivation has been suggested as a test of a species; but this is the very best means of inducing a wild plant to vary, as all cultivators know. The fixation of any variation is a matter of time. About five years may, perhaps, be regarded as the average period under cultivation in "fixing" races: but nothing is known about wild varieties. In either case the rule is that the environment must be constant.

Indefinite Variations, non-existent.—This is the second hypothetical source of new variations according to Darwinians.

With regard to all the offspring varying approximately alike and not "indiscriminately" (Romanes) or "indefinitely" (Darwin) when subjected to changed conditions of life, I wish to emphasise the fact most strongly that experiments show conclusively that if seedlings are subjected to a markedly different environment, when they grow up to maturity, the rule is, that all that do change, change in precisely the same way. They do not vary indefinitely among themselves; so that there is no material here—any more than with "individual differences"—for natural selection to act upon. Thus, in cultivating the wild parsnip or carrot, all the seedlings that change, do so by beginning to assume the same new characters—viz., an increased size with a greater fleshiness in the root, larger dimensions of the leaves, reduction of hair, &c., with a corresponding alteration in the anatomical structures.

So, too, if the seeds of an amphibious plant as *Ranunculus heterophyllus* be sown in a garden border, all grow absolutely alike

in adaptation to the aërial medium. Numerous other illustrations could be given.¹

I think it must be from not being aware of the abundance of evidence of this sort, that the idea arose that all the offspring did not acquire the same characters when the external conditions were changed.

Dr Wallace doubts it because, he says, "the argument is, that the same causes will always produce the same or closely similar results. But this is only true when the same causes act upon identical materials and under identical conditions."² Dr Wallace is mistaken in supposing that nature pays any attention to "individual differences" which occur between any number of plants of the same kind. It is not a question of argument, but of facts. These differences are of no moment whatever when self-adaptation is required to take place. The external influences cause all the individuals to change alike in the same direction, and utterly ignore the various dimensions among the "individual differences" described above. The result is that the same facies is acquired by all the individuals, though a new set of individual differences may readily be found among the individuals of the new variety.

Secondly, besides doubting the occurrence of the same definite variations in the whole of the offspring subjected to new but similar external conditions, Dr Wallace adds:—"It must do more than this, for it must produce a variation so exceptionally stable that it constantly recurs in all the offspring of successive generations, even though those offsprings are subjected to considerable change of conditions."³

But the stability of a species, I repeat, is a purely relative matter and depends upon time. Some plants are very plastic, others are not so, some variations may become very (but never absolutely) rigidly fixed, while others may refuse to be reproduced by seed with any certainty at all. Not only is this true when the plant is propagated by seed but it is also true for vegetative multiplication. Tulips, &c., introduced from the East, though they have presumably been constant in form for unknown ages, yet often become unrecognisable in three years though propagated by bulbils only; apple trees, though propagated by grafts alone have given rise to numerous varieties; even different kinds of apples raised on stocks, but grown in the same States of N. America, respectively, often bear fruit of approximately the same form.⁴ On the other hand the Jerusalem artichoke, asparagus, sea-kale and celery offer

¹ The reader is again referred to "The Origin of Plant Structures" for further details.

² *Loc. cit.*, p. 488.

³ *Loc. cit.*, p. 489.

⁴ "Bud Variation and Evolution," *Natural Science*, vol. vii., p. 103. An essay in Mr Bailey's work "The Survival of the Unlike," 1896.

but little variations to select from. Of common vegetables, parsnips, carrots, radishes, *Brassica oleracea*, &c., have supplied numerous varieties which come true by seed; though each may still furnish an improved "race."

Similarly, if a useless character be acquired among cultivated plants, not only may it occur in every individual but it may become hereditary and relatively fixed; just as in the examples of wild plants already mentioned. Thus, there is no special advantage in the mere variety of colouring of flowers as of pansies, nor in double flowers, nor in excess of neuter flowers of composites, nor in the abortive pedicels of the feather hyacinth, &c.

With regard to the fixation of characters, therefore, there is no absolute rule whatever, nor can we say why one plant is so plastic and another refractory.

Nature recognises no "must" in her processes.¹

Darwinism, an Unverified and Unverifiable Deduction.—It is a common statement that Darwin placed the Doctrine of Evolution on a scientific basis when he pronounced the theory of "The Origin of Species by means of Natural Selection." It is against this statement that I would venture to protest most strongly. To take the latest example, Ludwig von Graff says:—"The selection theory of the celebrated Englishman, Darwin, first based the idea upon a scientific foundation. The obvious phenomena of heredity and of variability are the foundations of his bold system, the axles of life's mechanism; and the motive power of this mechanism is the struggle of all living things for the preservation and procreation of life."²

Darwin's theory, however, as stated in the title of his book, "The Origin of Species by means of Natural Selection," is a pure deduction; and deductions (*i.e.*, *à priori* reasoning), though useful as working hypotheses are not scientific or useless, until they have been verified by induction and experiments.

The theory was based on two primary deductions; out of these secondary ones followed. They were, first, that "Individual Differences" could supply materials for natural selection to act upon; secondly, that when offspring of any species varied under the action of new conditions of life, they generally varied indefinitely, so affording fresh material for natural selection. It has been shown that both of these fundamental assumptions are groundless.

As an illustration of his deductive method of reasoning, let us take the following typical passage which states Darwin's theory clearly and concisely:—

¹ Dr Weismann says:—"Doubt is the parent of progress;" yet in about a page and a half of *Nature* (June 11, 1896), in an epitome of his theory, he uses the word "Must" fourteen times!

² *Natural Science*, vol. ix., p. 193.

"It may metaphorically be said that natural selection is daily and hourly scrutinising, throughout the world, the slightest variations; rejecting those that are bad, preserving and adding up all that are good."¹ This, as far as the origin of species is concerned, is a pure assumption; and what I contend for is, that since observation and experiment show conclusively that variations can arise rapidly under one's very eyes, there is no need to assume any other process whatever than the protoplasmic response to environments. Thus, rhizomes are often recognised as being of specific or other diagnostic value, but when an aërial stem is made to grow underground, its new growth at once begins to assume the characters of an ordinary rhizome. Roots, stems and leaves normally living submerged have characters which are at once more or less assumed by a terrestrial plant if it be made to grow in water, and *vice versâ*; or if a water plant send a shoot into the air the change is abrupt at the level of the water. Plants in damp places are often very different as a whole from those in excessively dry situations. Reverse their positions and each at once begins to assume the characters of the other as soon as they grow in response to their surroundings. If lowland plants or their seeds be grown in high alpine regions they at once assume the facies of normal alpine plants. The markedly peculiar features of desert plants at once begin to break down, when a normally desert plant is grown in ordinary soil, just as the wild carrot or parsnip may quickly acquire the characteristic features of the cultivated form.

If Darwin had fully realised the significance of these and such like facts, he could hardly have continued the above passage with the following words:—"We see nothing of these slow changes in progress, until the hand of time has marked the lapse of ages; and then so imperfect is our view into long past geological ages, that we see only that the forms of life are now different from what they formerly were."² That all this is due to natural selection is simply an unverified deduction.

Self-adaptation, by Response to the Definite Action of Changed Conditions of Life, the True Origin of Species.—That plants vary by self-adaptation to a new environment is proved by inductive evidence and amply verified by experiment.

Let me repeat.—The struggle for life is incessant. Apart from ill-luck, which applies to all alike, the weaker in constitution are often expunged, while the stronger survive and the general distribution of plants in time and space is the result. This however, as Darwin insisted, is a quite different thing from the origin of species.

The origin of species is due, for the most part, or as a broad

¹ "Origin, etc.," p. 65.

² "Origin, etc.," p. 66.

general rule : first, to migration and isolation from the parent type, with as much freedom from the struggle for existence as possible ; secondly, to self-adaptation by the inherent power of response in living protoplasm, excited by the physical influences of the new environment. The result is for the most part new structures in harmony with the new environment. If there be a thousand seedlings of one and the same plant which germinate and grow together, they will all put on, more or less, the same features under the same definite action of the same surroundings ; though individual differences will still be found among them as before.

Conclusion.—Lastly, the answer to the question which heads this paper is that natural selection plays no part in originating new varieties, nor is it required as “ means ” or an aid in the origin of species ; but is all-sufficient in the distribution of plants.

Now the above conclusion is practically admitted by Dr Wallace himself, in the following sentence :—“ Should they [fixed varieties of plants] be found to occur more frequently in other countries [*i.e.*, ‘ Representative plants,’ which are indeed innumerable] as varieties of birds, mammals, and reptiles, &c., occur in separate areas in North America — they may be usually explained as adaptations to very different climatic conditions, in which case the distinguishing characters will be utilitarian [or otherwise] and the local varieties will be really incipient species.” The passage I have spaced represents precisely the views expressed in this paper. Darwin, too, admits the possibility of the origin of species without the aid of natural selection. His words are as follows :—“ By the term definite action, I mean an action of such a nature that, when many individuals of the same variety are exposed during several generations to any change in their physical conditions of life, all, or nearly all the individuals are modified in the same manner. A new sub-variety would thus be produced without the aid of natural selection.”¹

Lastly, this was the conclusion of Mr Herbert Spencer, in 1852, seven years before Darwin and Dr Wallace superadded natural selection as an aid in the origin of species. He saw no necessity for anything beyond the natural power of change with adaptation ; and I venture now to add my own testimony, based upon upwards of a quarter of a century’s observations and experiments, which have convinced me that Mr Spencer was right and Darwin was wrong. His words are as follows :—“ The supporters of the development hypothesis can show . . . that any existing species, animal or vegetable, when placed under conditions different from its previous ones, immediately begins to undergo certain changes of structure fitting it for the new conditions . . . that in the

¹ “ Animals and Plants under Domestication,” vol. ii., p. 271.

successive generations these changes continue until ultimately the new conditions become the natural ones. . . . They can show that throughout all organic nature there is at work a modifying influence of the kind they assign as the causes of specific differences; an influence which, though slow in its action, does in time, if the circumstances demand it, produce marked changes."¹

All, therefore, I ask of my readers is to weigh well the evidence that has been again of late years brought forward in favour of adaptation in lieu of natural selection as the means by which varieties originate; and not to be biassed by, it may be, many years of conviction that Darwinism was all-sufficient. It is solely a question of evidence, and as the doctrine of evolution ultimately broke down men's faith in Creation by "Fiats" and the Argument of Design, so it is hoped that before this century closes, it will be seen that Darwin's deduction of "The Origin of Species by Means of Natural Selection" was a most unfortunate one, as it is quite incapable of verification; while the conclusion of Mr Herbert Spencer has been abundantly verified, both by inductive evidence and experimental proof.

GEORGE HENSLOW.

¹ "Essay on The Development Hypothesis," 1852.

III

Reproductive Divergence: An Additional Factor
in Evolution

SOME ten years ago the late G. J. Romanes propounded his theory of Physiological Selection,¹ which was founded on the fact that certain individuals of a species, though fertile with some, may be perfectly sterile with other individuals. Supposing such incompatibility to run through a whole race, then these varieties, separated by a physiological barrier from the rest of the members of the species, would be preserved, and might vary independently, and so become gradually split off from the parent species in respect of other characteristics as well.

This theory has not been generally received, and Wallace, in particular, has demonstrated² very clearly that in the form propounded by its author the theory cannot stand. Nevertheless, the theory served to draw attention to the importance of variations in the reproductive powers of organisms as a factor in evolution, and to emphasise certain unexplained difficulties in the theory of natural selection, more especially with reference to the sterility of first crosses between species, coupled with the fertility of those between varieties, the swamping effects of intercrossing, and the frequent inutility of specific characters.

In the present paper I wish to bring forward a theory which is also concerned with variations in the reproductive powers of organisms as an important factor in evolution, but which is essentially different from that propounded by Romanes. This theory may be enunciated as follows. Supposing that among the members of any species, those individuals, more alike, in respect of any characteristic, such as colour, form or size, are slightly more fertile *inter se* than less similar individuals, it necessarily follows that in the course of succeeding generations the members of this species will diverge more and more in respect of the characteristic in question, whereby ultimately the original species may be split up into two or more fresh species.

This principle I have ventured to call "Reproductive Divergence." It is best illustrated by a concrete example. Supposing

¹ *Journ. Linn. Soc. (Zool.)*, vol. xix., p. 337, 1886.

² "Darwinism," p. 180.

that in the Lepidopterous *Ithania urolina*, an insect found in the Amazon valley, small individuals were slightly more fertile with other small individuals than with larger individuals, whilst these were also more fertile *inter se*, then it would follow that fewer individuals of intermediate size would be produced, and in course of time the species would be split up into a small and a large variety. These varieties would continue to diverge as long as the principle of "reproductive divergence" was acting, till at length they might become differentiated into two mutually sterile species. Supposing, on the other hand, this variation in fertility were correlated with slight differences of colour, then in course of time varieties differing in respect of colour would be produced, or if it were correlated with both size and colour, varieties differing in respect of both characteristics might be produced. As a matter of fact, this insect does actually occur as four distinct varieties, differing in colour, form and size,¹ though whether in consequence of the operation of reproductive divergence, it is of course impossible to say.

It will be observed that the theory enunciated is made up of two parts, the first of which can only be verified by experiment, whilst the second is the statement of a fact, which is capable of mathematical demonstration. This we will now proceed to afford.

Let a certain number of individuals of a species, say 900 males and 900 females, be divided up into three groups, according to their size. Let there be 300 small males, *S*, 300 medium sized ones, *M*, and 300 large ones, *L*. Let the 900 females be similarly divided up into the three groups, *s*, *m* and *l*. In order to maintain the number of individuals constant in each generation, let it be granted that any number of males and females breeding together give rise to the same number of males and females. Then if these 900 males and females be allowed to breed together, on an average 100 small males, *S*, will breed with 100 small females, *s*, and 100 male and female offspring, *Ss*, will arise. Similarly also there will be 100 male and female offspring, *Sm*, and 100 *Sl*. Again, with reference to the medium sized males, there will be 100 male and female, *Ms*, *Mm*, and *Ml* offspring; and with reference to the large sized males, 100 male and female, *Ls*, *Lm* and *Ll* offspring. Now let it be granted that the offspring *Sl* and *Ls* are of the same size as *Mm*, and that *Sm* and *Lm* are respectively of the same size as *Ms* and *Ml*. Then as the result of the chance breeding of the 900 males and females, we shall have the following numbers of individuals of each sex formed:—

100 *Ss*, 200 *Sm*, 300 *Mm*, 200 *Ml*, 100 *Ll*.

Now let us suppose that the comparative fertility of the various sized individuals is slightly changed, so that the principle of "repro-

¹ H. W. Bates, *Trans. Linn. Soc.*, 1862, p. 545.

ductive divergence" may come into operation. Let 100 individuals breeding with similar sized individuals, give birth to 120 offspring of either sex instead of 100, whilst 100 individuals breeding with moderately smaller or larger individuals (*i.e.*, *M* and *m* breeding with *s* and *S* or *l* and *L*) give birth to, on an average, only 95 offspring, and 100 individuals breeding with considerably smaller or larger individuals (*i.e.*, *S* or *L* breeding with *l* or *s*) give birth to only 80 offspring of either sex. Then it will be found that the 900 males and females breeding together will give birth to the following:—

120 *Ss*, 190 *Sm*, 280 *Mm*, 190 *Ml*, 120 *Ll*.

That is to say, whilst the largest and smallest individuals have increased in numbers by 20 per cent., the medium sized ones have decreased by 7 per cent., and the ones intermediate between these by 5 per cent. The fact that the medium sized individuals have decreased in number, in spite of the 100 *M* and *m* individuals which breed together having produced 120 *Mm* offspring, is of course due to the fact that only 160 *Mm* individuals are produced by the crossing of the 100 *S* and 100 *L* individuals with the 100 *l* and *s*.

In a similar manner, in succeeding generations, the numbers of individuals intermediate in size will gradually become smaller and smaller, whilst those of the extreme ones will increase. But, it may be said, even then the two varieties thus formed will not differ in size to a greater extent than the extreme individuals in the original 1800 taken. This is not the case. Thus supposing the three groups of individuals were respectively on an average 65.5, 68.5 and 71.5 inches in length, the extremes among the small individuals being 64 and 67 inches, those amongst the medium 67 and 70 inches, and those among the large 70 and 73 inches. Then suppose that by the principle of reproductive divergence the individuals were separated into two groups of an average of 64 and 73 inches in length. Then it follows that these groups would (approximately) contain individuals varying between 62.5 and 65.5 inches, and 71.5 and 74.5 inches respectively. That is to say, considerably smaller and considerably larger individuals would be formed than were originally present. Also if the principle of reproductive divergence continues to act amongst the two varieties of the original species formed, the individuals will continue diverging more and more in respect of this characteristic, with which the reproductive power of the organism is correlated. Also, if it be granted, that on an average, the more widely any two individuals differ in size, the greater is the relative degree of sterility between them, it follows that in course of time the individuals of the two varieties will become mutually sterile: or in other words, that from the original species two new species will have arisen.

Having demonstrated the correctness of the second part of the

hypothesis enunciated, it remains to bring forward experimental evidence of the validity of the first part—i.e., it is necessary to prove that in some cases more closely similar individuals of a species show greater mutual fertility than less similar; in other words, that there may be a partial sterility between varieties. On this point Darwin has collected a considerable amount of evidence in his "Variation of Animals and Plants under Domestication."¹ A few of the cases mentioned there may be now cited. Thus Gärtner found that a variety of dwarf maize, bearing yellow seed, showed a considerably diminished fertility with a tall maize having red seed, though both varieties were perfectly fertile when crossed *inter se*. Again, in the genus *Verbascum*, numerous experiments were made by Gärtner with the white and yellow varieties of *V. lychnitis* and *V. blattaria*, he finding that crosses between similarly coloured flowers yielded more seed than those between dissimilarly coloured flowers. These experiments have been repeated and extended by Scott with confirmatory results. Again, Girou de Buzareingues crossed three varieties of the gourd, and concluded that their mutual fertilisation is less easy in proportion to the difference which they present. Still again, the blue and red varieties of pimpernel, which are considered by most botanists as varieties, were found by Gärtner to be quite sterile when crossed.

With regard to members of the animal kingdom, there is very little evidence. Such as there is, is related only to domesticated animals, and can be at once objected to on the ground that it merely shows that the animals in question are descended from two or more distinct species. Thus Youatt² states that longhorn and shorthorn cattle, when crossed, show a diminished fertility. This statement has, however, been denied by Wilkinson.

The evidence determinable from certain anthropological data is, on the other hand, of more value. Thus Professor Broca has brought forward evidence³ that some races of man show diminished fertility together. Again, according to statistics collected in Prussia from 1875 to 1890, it was found that Protestants, Catholics and Jews, marrying among themselves, had, on an average, respectively 4·35, 5·24 and 4·21 children. When, however, the husband was a Jew and the wife a Protestant or Catholic, the numbers of children were only 1·58 and 1·38 respectively; and when the wife was a Jewess and the husband a Protestant or Catholic, only 1·78 and 1·66 respectively. Whether this apparent partial sterility was due to differences of race or to social reasons it was impossible to say.⁴ Still again, from the natality tables of Körosi,⁵ which are calculated

¹ 2nd ed., vol. ii. p. 82.

² "Cattle," p. 202.

³ "On the Phenomena of Hybridity in the Genus *Homo*," 1864.

⁴ Quoted from Mayo Smith's "Statistics of Sociology," p. 115.

⁵ "Phil. Trans.," 1895, B. 781.

from the marriage statistics of 81,000 couples in Buda-Pesth, it is possible to obtain evidence supporting our theory. Thus from these figures one may see that parents of similar ages are more fertile *inter se* than parents of dissimilar ages. With very young mothers the most fertile fathers are, on an average, from three to six years in advance as to age; but with increasing years of the mothers the ages of the fathers become less and less in excess, till at about thirty years of age they coincide. At greater ages they gradually become slightly in defect. Though this greater mutual fertility of individuals like in respect of age can be of no influence in modifying the species, or splitting it up into varieties, yet it gives us reasonable ground to suppose that the fertility may also be found on examination to be greater with individuals similar in respect of some other characteristic. In such a case there would be a tendency for two or more varieties to be formed, unless there were some other agency counteracting it.

It will be seen that the evidence adduced in favour of a partial sterility sometimes existing between varieties of a species is, in the case of animals at least, very meagre. The reason of this is not far to seek. Thus wild animals, when placed in confinement, will not, in the majority of cases, breed at all. Domesticated animals, on the other hand, do not afford evidence of much value, for the reason given above. Also, it is generally held that domestication of itself tends to increase fertility, and so would overcome any tendency to sterility of varieties.

In order to obtain evidence as to the existence of a diminished fertility between varieties, I have made a considerable number of observations on the effects of crossing the various colour varieties of the sea urchins, *Sphaerechinus granularis* and *Strongylocentrotus lividus*, and have found that from a given number of ova the number of blastulae and the number of larvae subsequently produced are appreciably smaller for crosses of dissimilar colour varieties than for those of similar ones. Also, the larvae produced are, on an average, about 5 per cent. smaller. As, however, it will be necessary to repeat these observations a large number of times before the proof of such a partial sterility can be considered quite unexceptionable, and as moreover I hope to be able to make similar series of observations among other classes of the Animal Kingdom, it would be premature at this point to refer to these investigations at any greater length.

It should be borne in mind that the theory of Reproductive Divergence does not require that there should be a partial sterility between the varieties of species in all cases, or in even the majority of cases. It merely premises that such sterility does exist in certain instances, and that in these the members of the species will gradually become more and more divergent in respect of one or more

characteristics, unless of course other causes are at work counter-acting its influence. Probably in the majority of cases, in most stable species in fact, there is no such variability of fertility between slightly differing individuals, and hence there is, from this cause, no tendency to the formation of more or less distinct varieties. Very probably, however, there is a latent possibility of such a variability of reproductive power arising in almost any species, when for instance some of its members are exposed to fresh environmental conditions, in consequence of migration or change of climate. If this is so, then a species will tend to split up into varieties just at the most opportune moment, the varieties thus formed becoming by the action of Natural Selection gradually more and more adapted to their surroundings, and so fresh species produced. That change of environmental conditions has a very great influence on the reproductive powers of both animals and plants is a well-known fact, and one on which Darwin has collected much valuable evidence.¹

It now remains to be demonstrated how the theory of reproductive divergence can successfully account for some of the chief objections which have been brought against the theory of Natural Selection, objections indeed which have been of considerable weight in deciding many scientists against the doctrine of the all-sufficiency of Natural Selection as the cause of Evolution.

The fact of the very general infertility of crosses between species and their hybrid offspring, coupled with that of the fertility of crosses between varieties, and of their mongrel offspring, was recognised by Darwin as a formidable objection. Though this distinction between species and varieties is now recognised as not of such universality as it was formerly believed to be, yet it is still admitted to be a difficulty hitherto by no means adequately accounted for. The theory of reproductive divergence offers a most satisfactory and convincing explanation. Thus according to it, as we have seen, varieties and ultimately new species have, in many cases at least, been formed by the operation of a slight and accumulating sterility between unlike individuals, whereby two or more groups of individuals become more and more segregated, and so capable of undergoing independent variation. This divergence of species takes place quite independently of Natural Selection, but this principle can always be exerting its action at the same time, whereby the new or modified characteristics produced can, if useful to the species, be accumulated and rendered better adapted to the environmental conditions. Whether the very general sterility of crosses between species is due originally in most or in all cases to reproductive divergence, or whether it came into operation but seldom, it is not as yet possible to say. If extended series of experiments show that it is in fairly

¹ "Variation of Animals and Plants under Domestication," vol. ii., pp. 130-149.

frequent operation in those species having a tendency to split up into varieties, it may be concluded that it was, and is, an extremely important factor in the production of sterility of crosses between species. Thus, as has already been mentioned, it is not supposed that reproductive divergence comes into effect in fixed and stable species, but only in those which, probably in consequence of changes of conditions of environment, are in the course of splitting up into varieties and new species.

Connected with the fact of the general mutual fertility of varieties, is that of the swamping effects of intercrossing. Thus if varieties are perfectly fertile with the parent form, it is difficult to see how they can ever establish themselves as incipient species, unless they become separated from the parent form by a geographical or other barrier. If, however, these varieties have arisen in consequence of the operation of reproductive divergence, it is obvious that they can preserve their characteristics unobliterated, and continue to exist in the same region as the parent form.

One of the most important objections to the doctrine of the all-sufficiency of Natural Selection as a cause of evolution, is that of the very frequent inutility of specific characters. Some naturalists, especially Wallace, are inclined to maintain that all specific characters are of use, and that it is only due to our ignorance that they appear to us useless. It is a more generally received opinion, on the other hand, that these characters can frequently be of no useful purpose to the organism, and must therefore have originated by some other means than Natural Selection. Darwin himself was fully alive to this objection, and considered that such useless specific characters might owe their origin to the correlation of organs, or to the laws of growth, and to so-called spontaneous variability.¹ These seem but very inefficient causes for such frequently occurring effects, and hence there is a strong *prima facie* evidence that some other principle is at work. The principle of reproductive divergence offers a satisfactory solution of the problem. Thus, as we have seen, by means of it species are caused to diverge in respect of one or more characteristics, and so fresh or altered characteristics can be originated without the influence of Natural Selection. To take a concrete instance, one mentioned by Bateson.² The commonest forms of lady-birds are the small *Coccinella decempunctata*, and the larger *C. septempunctata*. The small insect is very variable in colour and the pattern of its colours, whilst the large is almost absolutely constant in these respects. This difference in specific characters may have originated by a common parent form having had its colour marking, and also the size of the individuals to a slight extent corre-

¹ "Origin of Species," 6th ed., p. 171.

² "Materials for the Study of Variation," p. 572.

lated with reproductive power. After the splitting up of this parent form into a large and a small species, in each of which the colour marking was invariable, the variations in fertility in the larger form, as correlated with colour marking, may have ceased, owing perhaps to the conditions of environment having changed from a variable to a more constant state, and the species would now become constant in this respect. The smaller form, on the other hand, may still be in the course of splitting up into two or more other species, differing in respect of colour marking, and maybe, of other characteristics.

Another not fully explained question with regard to the origin of species is that of the divergence of character. Why is it that in the course of evolution, species have widened out into diverse branches, and have not continued in merely linear series? This question of divergence has been examined somewhat fully by Gulick.¹ Darwin seeks to answer the question "from the simple circumstance that the more diversified the descendants from any one species become in structure, constitution, and habits, by so much will they be better enabled to seize on many and widely diversified places in the economy of nature, and so be enabled to increase in numbers."² As Romanes points out,³ this argument is, however, assailable in one particular, *i.e.*, it ignores the fact of the swamping effects of intercrossing. Thus, in Darwin's own words, it is where specific forms "jostle each other most closely" in an overstocked area that Natural Selection will be enabled to act most favourably on any members which may depart from the common type. Now, any varieties formed under these conditions by the splitting up of a species will be almost inevitably swamped by their mutual intercrossing, unless there be some degree of sterility between them. Under these conditions, therefore, reproductive divergence can act at a great advantage, as not only can it originate varieties, but by the mere fact of so doing it ensures these varieties not being eliminated by the swamping effects of their mutual intercrossing.

It is unnecessary on this occasion to show how the theory of reproductive divergence may be applied to the other questions and difficulties connected with the theory of Natural Selection as an explanation of the mechanism of the origin of species. Suffice it to say that to some points in connection with Geographical Distribution, with the origin of rudimentary organs and other questions, it offers most material aid. The objections to the theory itself, as far as they present themselves to me, seem to be but few, and of but little weight. One of the most obvious is the frequently made statement, that crosses between varieties generally produce indi-

¹ *Journ. Linn. Soc. (Zool.)*, vol. xx., p. 189.

² "The Origin of Species," p. 87.

³ *Loc. cit.*, p. 385.

viduals of greater vigour and fitness than the parents. As far as I am aware, there is no evidence to show that this greater vigour is the result of the differences of morphological form, but rather that it is due to the individuals being descended from different stocks, whereby the evil effects of in-and-in breeding are avoided, or to being exposed to differences of environmental conditions, whereby they may perhaps be rendered physiologically unlike individuals to a slight degree, rather than morphologically unlike. That mere exposure to differences of environmental conditions may be sufficient to give rise to a vigorous race even when this is propagated by the closest in-and-in breeding, is shown by the case of the rabbits on the Island of Porto Santo, all of which are descended from a single pregnant individual.

Another objection which might be raised is, that in the case of both plants and animals it has frequently been found that varieties showing considerable differences of external form are perfectly fertile *inter se*. Even if this is the case, it is no argument against the theory of reproductive divergence, for it was specially mentioned that this is not supposed to be invariably in operation when a species is in course of splitting up into varieties. At the same time, it may reasonably be doubted whether this statement as to the perfect fertility of varieties is a fact, because a very slight degree of sterility would easily escape notice unless extensive series of breeding experiments were made, and careful records kept.

H. M. VERNON.

IV

On the Restoration of some Extinct Reptiles

THE exhibition of large diagrams in museum cases has met with the disapproval of many who are in a position to give an authoritative opinion; but, by way of justification of such a practice, it may be pointed out that it frequently happens in a museum that, since it is only possible in rare instances to have cases specially made to accommodate definite series of specimens, spaces will occur which are a source of much trouble to the curators; and diagrams, from their elasticity of size, can always be relied upon to fill what must otherwise be left blank. It is just such a difficulty that has to be confronted in planning out some of the cases at the Natural History Museum. The wall-cases, for instance, on the south side of the third Bay on the left-hand side of the Entrance Hall are devoted to the elucidation of the more important features which are made use of in the classification of reptiles, and contain stuffed specimens, casts, and skeletons, articulated and disarticulated, of representative members of each order. But the cases are ten feet in height, and the upper compartments are too far removed from the eye of the observer, and too badly lighted, to admit of the recognition of much detail in the specimens exhibited there. The framework of the back of the case, also, is too slight to bear heavy specimens, and it is here, if anywhere, that the exhibition of wall-diagrams is justified.

As complete skeletons of extinct reptiles of such a size as to fit conveniently into these wall-cases without crowding out the recent members of the class, or being lost among them by reason of their diminutive size, are almost impossible to obtain; and as the disjointed parts of the skeleton of these extinct forms are efficiently represented either by actual specimens or by casts in the table-case, it was, when recently planning out this wall-case, considered sufficient for the purposes of the Index Collection to represent the Ornithosauria, the Ichthyopterygia, the Sauropterygia, and the Anomodontia by bold diagrams of the whole skeleton of one selected species of each, drawn to such a size as to fill the four top spaces. The diagrams, which have now been completed and are exhibited in the cases, measure about 27 inches in height, and 41 inches in breadth. They are bold outline dia-

grams, in black lines on a white ground, executed by Miss G. M. Woodward with the artistic skill and excellence of technique which invariably characterise her work.

The species chosen to represent the Order Ornithosauria is *Dimorphodon macronyx*, from the Lower Lias of Lyme Regis (Fig 1). Owen's well-known restoration of this species ("Liassic Reptilia," *Mon. Pal. Soc.*, 1870, pl. 20., and "Hist. Brit. Foss. Rept.," 1849-84, vol. iv., pl. 17), naturally formed the basis of the diagram, but the shapes and proportions of the bones were taken from the actual specimens, of which the Geological Department of the museum can boast a good many.¹ The correctness of Owen's restoration of the pelvis was severely criticised by Seeley in 1891

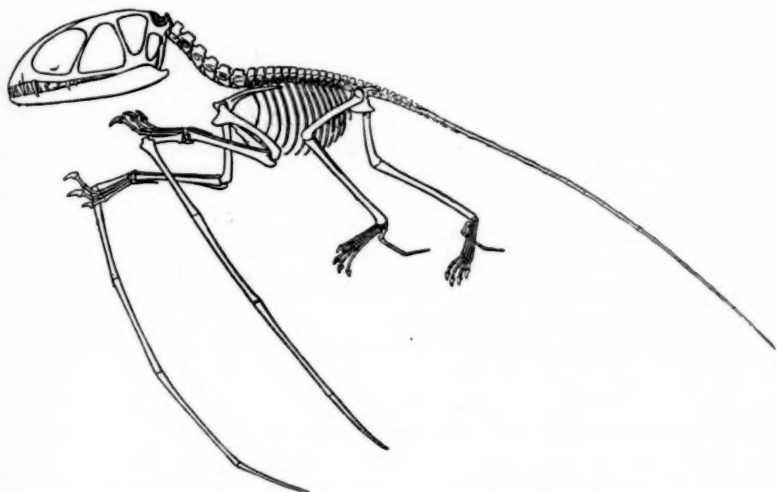


Fig. 1. *Dimorphodon macronyx*, from the Lower Lias of Lyme Regis. (x1).

(*Ann. and Mag. Nat. Hist.*, ser. 6, vol. vii., pp. 235-255), and recourse was had to figures 11 and 13 of this paper when drawing the pelvic region of the skeleton. The pteroid bone, or backwardly directed metacarpal of the rudimentary thumb, which is incorrectly shown on the ulnar side of the limb in Owen's figure, was introduced from the specimen (R. 1034) in the Geological Gallery, and the details of the caudal vertebrae from specimen (41346), figured by Owen in the "Liassic Reptilia" (pl. 19, fig. 4). Since the back part of the skull is crushed in the Natural History Museum specimens of *Dimorphodon*, the outlines of the quadrate bone and the supra-temporal and lateral temporal fossae were added from

¹ The more complete skeletons were described and figured by Buckland, Owen, and others, and references to the descriptions and figures are to be found in the *Brit. Mus. Cat. Foss. Reptilia*, part i., pp. 37-39.

Newton's figures of the skull of the allied genus *Scaphognathus* (*Phil. Trans.* 1888, B. pl. 77 and 78). There appears to be no palaeontological evidence to warrant the great length which Owen gave to the hindermost ribs in his restoration, and these have, therefore, been considerably shortened, so that the contour of the ventral abdominal wall now passes evenly from the ribs to the ischial bones of the pelvis. So little is yet known concerning the coracoids and sternum of Ornithosauria that, beyond representing the sternum as keeled and as articulating with the first few ribs, but little has here been attempted.

Of the four restorations which form the subject of the present article the greatest interest probably centres around that of *Ichthyosaurus*, inasmuch as the recent additions to our knowledge of this genus have rendered possible a very complete restoration. The species chosen is *Ichthyosaurus communis* from the Lower Lias of Lyme Regis, and the specimens which form the basis of the reconstruction are those in the Geological Gallery bearing the register numbers (41849) and (2000,1*). This is the same species as that of the well-known restoration of Owen's ("Anat. of Vert.," vol. i., 1866, p. 170). An illustrated summary of recent papers on the Ichthyopterygia has already appeared in the pages of this journal (Lydekker, *Nat. Sci.*, vol. i., 1892, pp. 514-521), and in this article is reproduced Fraas's figure of the wonderfully well-preserved specimen of *Ichthyosaurus quadriscissus*, showing the complete outline of the body and affording incontrovertible evidence of the presence of a bilobed tail with the vertebral column running down the ventral lobe, and the existence of a series of irregular integumentary fins along the back (Fraas, *Neues Jahrb. f. Mineral.*, 1892, Bd. 2, pp. 87-90). These details are reproduced in the present restoration (Fig. 2), and, while the proportionate size and the details of the paddle skeleton are taken from the specimens of *Ichthyosaurus communis* above specified, the postaxial flap of the paddle, not supported by skeletal parts, is added from Fraas's figure, from the museum specimen of *Ichthyosaurus intermedius* (R. 1664), described and figured by Lydekker (*Geol. Mag.*, dec. 3, vol. vi., 1889, pp. 388-390), and from Owen's figure of the paddle of *Ichthyosaurus communis* (?) ("Liassic Reptilia," part iii., 1881, pl. 28, fig. 5). The outline and details of the skull were introduced mainly from specimens (39492) and (R. 1164) of *Ichthyosaurus communis*, both of which exhibit a very complete side view of the skull. In none of the specimens of *Ichthyosaurus communis* at the Natural History Museum are the bones of the pectoral girdle undisturbed, so that in restoring this part of the skeleton the shapes of the constituent bones were taken from specimen (41848), but their mutual relations from the very complete girdle which the museum possesses of

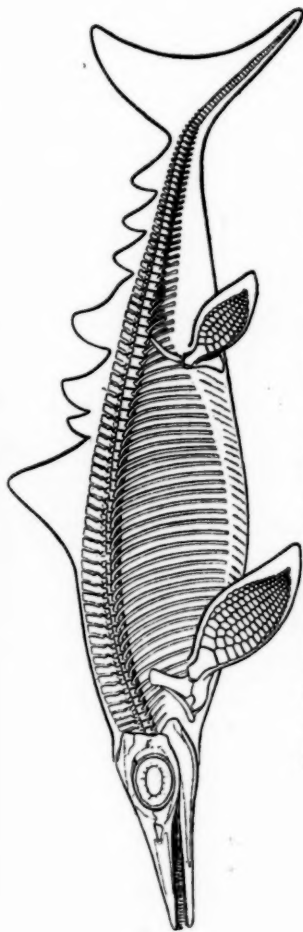


Fig. 2. *Ichthyosaurus communis*, from the Lower Lias of Lyme Regis. (x₁).

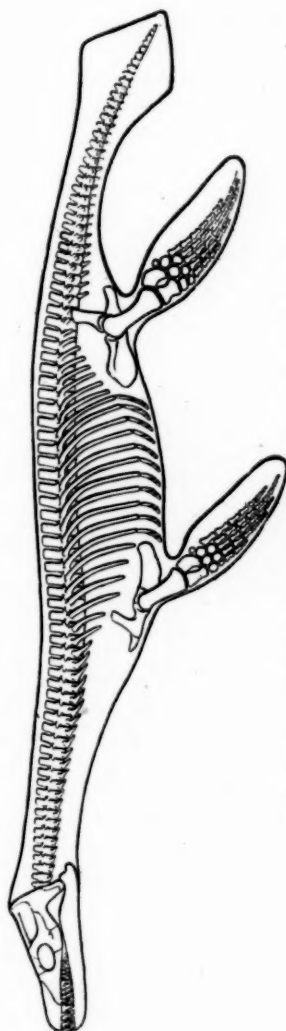


Fig. 3. *Plesiosaurus rostratus*, from the Lower Lias of Charmouth. (x₁).

Ophthalmosaurus icenicus (R. 2137), described and figured by Seeley (*Proc. Roy. Soc.*, vol. liv., 1893, fig. 1, p. 151). No such difficulty beset the restoration of the pelvis, since the parts are hardly at all displaced in specimen (41849).

The diagram of the Plesiosaur is mainly based on the splendid specimen of *Plesiosaurus rostratus* from the Lower Lias of Charmouth, Dorsetshire, exhibited in the Geological Gallery of the Museum, and bearing the register number (38525). This specimen was described and figured by Owen in his "Liassic Reptilia" (*Sauropterygia*, 1865, pl. 9), but it did not form the basis of his well-known text-book restoration of *Plesiosaurus* ("Anat. of Vert.," vol. i., 1866, p. 52), the species of which, according to Lydekker (*Brit. Mus. Cat. Foss. Rept.*, part ii., 1889, p. 121), is *macrocephalus*. The number of cervical vertebrae in *Plesiosaurus rostratus* is not definitely known. Owen put it down as twenty-four, but there were probably more, since in the specimen (38525) there are evidently some vertebrae missing after the seventeenth (see Lydekker, *loc. cit.*, p. 272). Judging from the shape and relations of the cervical ribs flexion of the neck must have been as difficult of achievement in *Plesiosaurus* as in our modern crocodiles, and so the vertebral column in the cervical region has been drawn nearly straight (Fig. 3), instead of being allowed the graceful sinuous curve which characterises Owen's figure. The outline of the body has been introduced from the figure given by Dames (*Abhandl. Königl. Akad. Wiss.*, Berlin, 1895, ii., p. 79); and special attention may be called to the shape of the tail fin, and to the presence of an integumentary extension of the paddle behind the part supported by the internal skeleton. The transverse temporal ridge at the back of the skull would probably not have influenced the general contour of the body to the extent suggested by the diagram. This improbability should have been avoided by making the vertebral column articulate a little higher up the occiput, and by putting the cranial axis more in a line with the cervical vertebrae.

The cranium of the specimen above mentioned is considerably crushed; therefore, while preserving the proportions of the cranial bones of this species, the actual details were added from the more perfect skull (49202) of the allied species *P. macrocephalus*, described and figured by Andrews (*Quart. Journ. Geol. Soc.*, vol. lii., 1896, pp. 246-253, pl. 9). The skeleton of the paddles in the specimen of *Plesiosaurus rostratus* is extremely well preserved, and nothing more was necessary than to copy the outlines of the constituent bones; but as the bones of the pectoral and pelvic girdles are disturbed, a certain amount of restoration was here inevitable, and the assistance derived from the perfect girdles of *Muraenosaurus plicatus* (R. 2428) and *Cryptoclidus oxoniensis*

(R. 2416) and (R. 2616) and the descriptions and figures of these specimens by Andrews (*Ann. & Mag. Nat. Hist.*, ser. 6., vol. xvi., 1895, p. 429; *ibid.*, ser. 6., vol. xv., 1895, p. 333; *Geol. Mag.*, dec. 4., vol. iii., 1896, p. 145) should here be acknowledged.

Pariasaurus baini was chosen to represent the Anomodontia, chiefly because of the completeness of the skeleton exhibited in the Reptile Gallery of the Geological Department of the museum. This skeleton (R. 1971), from the Karoo formation (Trias), was discovered by Prof. H. G. Seeley, near Tamboer Fontein in Cape Colony, and was described and figured by him in the *Phil. Trans.* (1892, B., pp. 311-370, pls. 17-19, 21-23). The Anomodontia constitute such a heterogeneous collection of reptiles that it would be difficult to say what species might be considered to be most typical of the Order. But the completeness of this specimen of *Pariasaurus* certainly renders it more suitable for the purpose in hand than any other Anomodont yet known. The diagram (Fig. 4) is not a

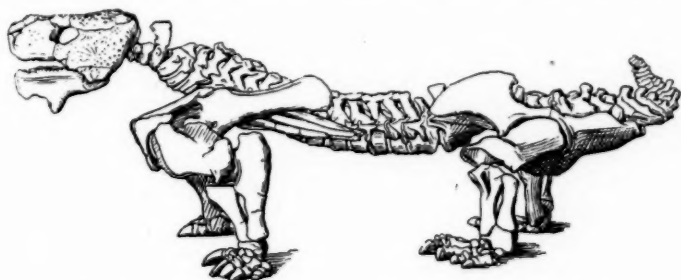


Fig. 4. *Pariasaurus baini*, from the Trias of Cape Colony. ($\times 1\frac{1}{2}$)

restoration in the same sense as the other three, because, in the first place, the completeness of the skeleton renders possible a very close adherence to nature, and, in the second, because, the whole of our knowledge of the species being derived from this one specimen, a reconstructed diagram would be less instructive than an outline drawing of the specimen boldly treated. The unimportant cracks in the bones shown in the large folding plate in the *Phil. Trans.* have been omitted, and a little diagrammatic cross-shading has been employed here to give the effect of distance, although it was not found necessary in the other three diagrams. The legs are shown articulating in the glenoid cavity and the acetabulum, as in the mounted specimen but not as in the plate; and the anterior cervical vertebrae, which during fossilisation were united into a block of extreme upward curvature, are given a more convenient disposition so as to articulate with the condyle of the skull,

which is not the case in the plate, nor in the specimen as now mounted.

My grateful acknowledgments are due to Sir William Flower, K.C.B., for permission to reproduce these figures for publication, and to Mr A. Smith Woodward, assistant keeper of the Geological Department, for sundry hints and advice during the construction of the diagrams.

W. G. RIDEWOOD.

V

The Facetted Pebbles of India

IT is now nearly forty years since the first account (1) of evidence of ice action in Palaeozoic times and within the tropics was published, and though the concept of a Permian glacial period is now one of the accepted results of geological research, the opposition to its acceptance is by no means dead. Some ten years ago this opposition received an access of strength by the arrival and exhibition in England of certain peculiar fragments of rock, first discovered by Dr Warth (2) in the Permian boulder beds of the Salt Range, which did not merely show a striation like that produced by glaciers, but bore several surfaces or facets which met in obtuse angles, and sometimes completely surrounded the stone. A number of these were sent home, unaccompanied by stones of other types, and an idea seems, perhaps not unnaturally, to have sprung up that these were the normal type of boulder, and not, as was the case, curiosities which were strange to geologists in India, and sent by them to their colleagues in Europe, with a view to enlightenment as to the mode of origin of a feature with which they were not acquainted as a result of ice action.

Specimens were exhibited at the Geological Society (3), the British Association (4), and elsewhere, and the general opinion may be expressed in the words of a letter by Dr W. T. Blanford to the *Geological Magazine* (5), that "the great difficulty in accounting for the origin of these facetted blocks is that whilst the smoothed surfaces are in every respect similar to those on stones worn by glacial action, no fragments from moraines, from boulder-clay, or from other glacial deposits, are known to exhibit the peculiar facetting characteristic of the present specimens."

Such was the general opinion held by most, if not all, of those who saw the specimens, and in the museum at Zurich one of these very facetted stones may be seen, with an endorsement on the label, by Professor Heim, to the effect that he had seen nothing like it in recent glacial deposits.

In these circumstances, the facetted stones being supposed to be the evidence on which was based the claim for a glacial origin of the beds in which they were found, it was natural that the opposition to the claim should be strengthened. In reality, however, the

supposition that these faceted pebbles were in some way the result of ice action was based on the fact that they were found in beds which, on quite independent grounds, were believed to be of glacial origin, and this belief would have been in no way affected if the faceted stones had been shown to owe their peculiar form to any other agency than ice.

All this while, however, there was on record the description of boulders of precisely similar character in glacial boulder clays of Post-Tertiary age. In 1879 Professor Credner published an account of the scratched stones found in the neighbourhood of Leipzig, (6) in which he mentions three types; the first being those on which a flat surface had been ground away on one side; the second comprising those on which two or more such surfaces are found meeting in obtuse angles; the third, those which show no facets, but are of a rounded or sub-angular form, and bear grooves and scratches scattered over their surface. It would be impossible to give a better classification of the stones found in the boulder beds of the Salt Range, and the closeness of resemblance is only enhanced when Professor Credner's detailed description is read.

This account appears to have been overlooked by all those who saw the Salt Range specimens, for which small blame can be laid, as the volume of glacial literature is so vast that the greater part must remain unread—even by those who devote themselves specially to this subject—and the paper might have remained unnoticed in this connection had it not been accidentally stumbled on while a very different line of research was being pursued. Struck with the light it threw on the origin of these curious pebbles I wrote to Professor Credner asking for further particulars, and in reply was informed that in the collection of the Saxon Geological Survey there are a large number of ice-worn stones showing two or more facets, meeting at an angle, and that in some these facets were distributed round the whole circumference of the stone. He also informs me that after a comparison of the specimens in Leipzig with the figures and descriptions of Drs Warth (2) and Noetling (7), he considers that their nature as glaciated fragments of the same character as those of the "gründ-moräne" of the northern ice-sheet is beyond doubt.

From this it is evident that we have, in Post-Tertiary glacial deposits, ice-worn fragments showing all the peculiarities of those found in the Permian boulder beds of the Salt Range, and with this the last objection to accepting their glacial origin should disappear.

R. D. OLDHAM.

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SOME NEW BOOKS

A FRENCH TREATISE ON ZOOLOGY

TRAITÉ DE ZOOLOGIE CONCRÈTE. Vol. I. La Cellule et les Protozoaires. By Yves Delage and Edgard Hérouard. Pp. xxx. 584, with 870 col. figs. Paris: Schleicher Frères, 1896. Price 25 francs.

THIS is the first instalment of a work which, if it finishes as it has begun, will be of the greatest value, since it combines completeness and erudition with a method of treatment at once highly original and well adapted to the end in view.

The primary object of the authors is to smooth the path of the student and to help him in his difficulties, and in their preface they are at pains to explain how it is intended to bring about this result. Every one knows how difficult it is, when commencing the study of a group of animals with the help only of an ordinary text-book of comparative anatomy, to apply the more or less vague generalities of which such works are composed to the case of a particular form. The usual method of describing a group of animals in the text-books or treatises on zoology is to commence with a chapter or chapters in which the comparative anatomy of the group is described organ by organ in a purely abstract manner—that is to say, without reference to the remaining organs of the body. This is followed by a systematic portion in which the families or genera are catalogued and distinguished by means of their external characters. The great defect of this mode of treatment is the want of any proper link between the abstract and the concrete, between the general and the particular. The beginner who is as yet unfamiliar with the group in question finds an extreme difficulty in forming a clear idea of how a particular form is organised in its entirety, since he has to combine in his mind a brief summary of its external characters with the rather vague mental image of its anatomy which he constructs by wading through the comparative chapters and picking out such portions as may apply to the form under consideration. Hence text-books of this class, though extremely valuable to the advanced student or teacher as works of reference, are confusing to the learner, who requires above all things something real and concrete, upon which to found his general notions.

It is not every student who has the time or opportunity to obtain the empirical basis so necessary for a clear grasp of the main principles, by consulting the special memoirs or monographs dealing with the forms he is studying, and in order to help him out of the difficulty a large class of practical text-books of zoology has sprung up in recent years, in which particular forms are chosen as typical examples of the larger systematic groups and described in great detail. In this way a division of labour has come about whereby the treatise of comparative anatomy is supplemented and elucidated by

the concrete examples of the practical text-book. It is, however, only to a limited extent that such co-operation is possible or practicable. The number of types which can be described within the limits of a practical text-book must be necessarily few by comparison with the ground covered by the more abstract treatises, and illustrative only of the greater systematic divisions. The design of our authors is an ambitious one. It is nothing less than to effect a compromise, so to speak, between the abstract and the particular, and to impart a general knowledge founded upon judiciously constructed, concrete examples.

A complete knowledge of a natural group of animals might be supposed attainable only by a separate description of each of the species in it. But allied species and even genera only differ amongst themselves by secondary characters, and it is not until we come to families or orders that we find anatomical characters of sufficient importance to warrant detailed treatment in the limited compass of a text-book. Hence for each such systematic division the authors propose to commence with the description of a generalised type, in which the characters of the subgroup—in most cases a suborder—shall be found combined, and then to proceed to point out how the various forms comprised in the subgroup differ severally from the essential type. But such generalised and fundamental types are to be found but rarely in nature. The authors have therefore invented and constructed a morphological type for each suborder, a fundamental form "which summarises in itself that which is common to all the actual forms of the group, or which is presented as a simple initial form, from which the others would be derived by progressive complications." In this way it is possible to present general notions in a concrete form. It might be objected that the morphological types are not real but represent to a certain extent ideal abstractions. In answer to this it is pointed out that the term concrete does not mean real. "A type may be concrete even though it is ideal. What does it matter to a student when he reads a precise description with the indication of all the organs and of their relations, whether the being thus described really exists in nature or whether it represents only the mean, we might almost say the composite portrait, of a small group of real beings? The idea he will obtain of the being described, and later of the entire group, will be none the less precise and none the less accurate."

For the reasons that have just been set forth, Messrs Delage and Hérouard call their work a treatise of concrete zoology, as opposed to the more abstract zoology of the ordinary text-books. They claim, and we think justly, to have helped the student over one of his greatest difficulties, though as they acknowledge, the Protozoa are scarcely a fair test for the efficiency of the method on account of their simple structure, and we are begged to suspend our final judgment until the appearance of the volumes to follow. At the same time the great store of information brought together in a most painstaking and laborious manner renders the work very useful to others than beginners, chiefly on account of the simple and methodical arrangement that has been adopted, and the consequent ease with which any required facts can be hunted down.

The present volume contains two parts, the first dealing with the cell and its functions, the second with the Protozoa. In the first part the authors give a review of general cytology, and deal with the vexed questions of protoplasmic and nuclear structures. Here, as they admit, they are often on very controversial ground, and experts would find much to criticise and to dispute in the opinions put forward. It cannot, however, be laid to the charge of our authors that they have neglected or passed over other views, though their criticisms upon them are occasionally perhaps rather one-sided; for having in view once more the exigencies of the student they have divided the work into two parts, one printed in large type composing the main text, the other in small type contained in the footnotes. In the former the objects are described in a simple and straightforward manner from the point of view taken by the authors, while to the footnotes are relegated the more controversial subjects as well as details concerning the less important or doubtful genera and similar matters. In this way the work is rendered extremely complete, and while on the one hand the student is treated to a clear and continuous, if at times dogmatic, *exposé* of the subject, he is enabled, on the other hand, to greatly extend his knowledge, if he wish, by means of the references and discussions in the footnotes.

The portion of the work dealing with the Protozoa contains a mass of information which it would be impossible to criticise in detail. We must, however, take exception to one innovation which has been introduced into this work, namely, the manner in which the authors have changed the names of the groups, in the attempt to introduce one uniform system of terminations for the equivalent taxonomic subdivisions. The results have been in some cases almost disastrous; we can hardly recognise such familiar groups as the Flagellata and Ciliata when we see them written as "Flagellia" and "Ciliae" respectively. In science a very good excuse is always necessary before the alteration of well-established names can be permitted. In the present case it is again solicitude for the student which is responsible for this well-meant but, we think, injudicious reform. It is supposed, for instance, that to make the names of classes end in *ia*, and subclasses in *iae*, in all cases, will tend to clearness. Not only, however, is this alteration of names rather confusing, especially to the beginner, but it involves the assumption, which can scarcely be maintained, that the various categories known as classes, subclasses, orders, and so forth, are of the same taxonomic value in all groups. The fact alone, however, that in the classifications of different authors, different names are given to equivalent divisions, is a sufficient refutation of this view, for where one author has a subclass divided into orders, another may have an order divided into suborders. It is, therefore, rather premature to coin a uniform termination for subclasses or orders until the value of these categories is more fixed. But further, Messrs Delage and Hérouard have given new names in their scheme to just those taxonomic categories for which, being of lesser and therefore of more definite value, the almost universal custom of naturalists has already established a uniform terminology. Nearly everywhere now names of families are made to terminate in *idae* and subfamilies in *inae*; yet our authors choose to employ the termination

idae for suborders and *inae* for families. The alteration in this way of an already uniform and established system of nomenclature seems to us quite unjustifiable.

In the classification of the Protozoa we notice a certain number of new names, some of which are coined for the purposes of new classifications, while others are older groups renamed. The Sporozoa are divided into two subclasses (1) Amœbogeniæ (nov.) with amœboid sporozoites, and containing the Myxosporidia, and (2) Rhabdogeniæ (nov.), with sporozoites of definite form, including the remainder of the class. The Rhabdogeniæ are further divided into the two orders, Dolichocystida (nov.), comprising the Sarcosporidia, and Brachycystida (nov.), which includes the remaining forms, namely, the Gregarinidae, Coccidiidae, Haemosporididae (*Drepanidium*, etc.) and Gymnosporididae (*Haemamoeba* and others). In the Ciliata Stein's four orders are maintained. The Holotricha are divided into Gymnostomidae, corresponding to Bütschli's Gymnostoma, and the Hymenostomidae (nov.), corresponding to Bütschli's Trichostoma Aspirotricha. The Peritricha are divided into Scaiotrichidae (nov.), comprising Bütschli's Lienophorina and Spirochonina, and Dextotrichidae (nov.) = Vorticellina. In the former the adoral zone of cilia has a sinistral ($\Sigma\kappa\alpha\iota\acute{o}\varsigma$) twist, in the latter a dextral ($\Delta\epsilon\acute{\xi}\iota\acute{o}\varsigma$).

The work is illustrated throughout by excellent diagrammatic figures, for the most part coloured, some even in as many as four colours. None of the familiar "vieux clichés" which persistently haunt one text-book after another, are permitted to intrude between these covers, all the figures being either specially constructed diagrams or else copied or modified from the original figures in the special memoirs. In short, the whole book is characterised throughout to a marked degree by one most precious quality, that of clearness and lucidity both in description and illustration. In conclusion, we congratulate Messrs Delage and Hérouard most heartily on the first results of their labours, and wish them all success in the great task which they have before them.

E. A. M.

GONIATITES IN THE BRITISH MUSEUM

CATALOGUE OF THE FOSSIL CEPHALOPODA IN THE BRITISH MUSEUM (NATURAL HISTORY). Part iii. Containing the Bactritidae and part of the suborder Ammonoidea. By Arthur H. Foord and George Charles Crick. Pp. xxxiv., 303. Published by order of the Trustees. London, 1897. Price, 12s. 6d.

THE first part of this Catalogue, published in 1888, and the second published in 1891, dealt with the Nautiloidea and were written by Dr Foord alone. That gentleman's removal to Dublin made some help imperative, and Mr Crick, the assistant in charge of the Cephalopoda in the Geological Department of the British Museum, has proved a worthy collaborator. The experience gained from previous work, combined with this fortunate co-operation, has brought the present volume, which treats of the older Ammonoidea, very near our ideal of what such a museum-catalogue should be. Some of these publications of the Natural History Museum have been important contributions to knowledge, but have left us still in the dark as to the precise extent or value of the Museum collections; others have dis-

played a marvellous zeal in the hunting up of ancient literature and the compilation of synonymies, but have not greatly assisted the student; others again have been dry lists of specimens, jotted down in haste and repented of at leisure, but having at least this merit, that they told us what material the Museum contained.

The present volume seems to us to combine the advantages, without the defects, of those predecessors to which we have referred. The descriptions of the species are most carefully drawn up, each being based, where possible, on examination of the type-specimen itself, and following a uniform plan, which greatly facilitates comparison. A useful diagram explains the terms employed. The difficulty of describing the all-important suture-line has been avoided by giving a tracing made from an actual specimen, if possible the type. There are also woodcuts of specimens, many of them from original drawings by Miss G. M. Woodward. The references to literature, in the form of lists of synonyma, are carefully done, but occupy a disproportionate space. When a species has never received more than one specific name, *e.g.*, *Prolecanites becheri*, it seems unnecessary to trace this through all the obvious genera, such as *Ammonites* and *Goniatites*, to which it has been referred by older authors, including the compilers of text-books and nomenclators. The information is useful, but might be put in less compass. Finally, this is a true catalogue; every specimen in the Museum is mentioned in such a way that it can be identified, and the number under which it is entered in the Museum lists or registers is printed. Thus the foreign student can gauge precisely the wealth of the collection, can tell whether what he wants to see is contained in it, and on reaching the Museum can ask for the definite specimen he requires.

One or two improvements may be suggested for future volumes of this and other catalogues. The statements of locality are misleading: under each species comes a series of statements made with reference to the species in general, including the usual size attained. After "Size" follow "Form. and Loc." These, however, refer not to the species, but to the particular specimens in the Museum. It would be better to give the general geological and geographical distribution of the species, and to refer to definite localities under the individual specimens, as is already done in cases where more than one locality is represented. It would be well to draw more forcible attention to the type-specimens, *e.g.*, by broad-faced type, also to distinguish cotypes, paratypes, and the rest. It is good to know the names of donors, especially when they are such men as J. E. Lee and John Roë; but it would also be good in other cases to know the names of those from whom specimens have been purchased, since these must often have been geologists of repute, whose statements of locality and the like would be of more value than those of an ordinary dealer or inefficient collector. It is sad to see how many specimens are entered with "*History unknown*," and of how many others "*Transferred from Mus. Pract. Geol.*" the necessary details are not recorded; but this is no fault of Messrs Foord & Crick.

A catalogue is not a text-book; nevertheless the Catalogues of the British Museum have come to be looked for by us outsiders as likely to introduce some improved system, and to unravel the tangle

of conflicting authorities. We look for some pronouncement on debated points, and for some clue through the maze that perplexes us. In these respects the present work leaves us unsatisfied. There is no exposition of the principles of the classification adopted; there are no keys; and there is little to indicate the relations of the species to one another. The authors suffer from an excess of caution: they tell us what Hyatt has written, what Haug thinks, what is the opinion of Branco, and what one will find in Zittel; but what their own views are, wild horses will not drag from them. It is the duty of people with such advantages as have our authors, not only to have opinions but to express them. It is not enough to tell us of so interesting a form as *Clymenia* that its derivation "is at present enigmatical"; it has been that for half-a-century. But one doubts occasionally whether even the authors know their own minds. There is a vast deal of quotation as to the systematic position of *Bactrites*, but where it is placed after all, we cannot understand. In Part I. of the Catalogue, Dr Foord inserted it among the Nautiloidea; in Part II. he said that he would refer it to the Ammonoidea; and now in Part III. it is hung up in the air, as though it were an Archicephalopod or a Schematic Mollusc. Again, among the quotations bearing on this, we find a passage from Hyatt and some of his figures; but we find no quotation of the destructive criticism of this passage published by Mr Crick himself, in conjunction with Mr Bather, in *Natural Science* for December 1894 (vol. v., p. 425). It is less strange, but quite as inexcusable, that there should be no reference to the important papers by J. M. Clarke in the *American Geologist*. There is always some excellent excuse for the suppression of evidence, and we shall no doubt learn that this is all for the good of the Government—or its officials.

It is curious, in a volume dealing with the *Goniatites*, to find no family Goniatitidae and no genus *Goniatites*. The type-species of *Goniatites* is the *Nautilites sphaericus* of Martin, a perfectly well-known form, which appears in this book as a *Glyphioceras*. There seems no room for doubt that *Glyphioceras* must rank as a synonym of *Goniatites*, since the latter has some sixty years' priority.

With the few exceptions mentioned, the volume is brought well up to date, and the care with which it has been compiled augurs well for the continuation of the series.

POPULAR NATURAL HISTORY

THE CONCISE KNOWLEDGE LIBRARY—NATURAL HISTORY. Edited by Alfred H. Miles. 8vo, pp. xvi. and 771, with 530 original illustrations. London: Hutchinson & Co., 1897. Price, 5s.

THIS volume is the first of a projected series, the purpose of which is shown in the title, and in the editorial preface. The volumes are intended to be "concise and popular . . . at once accurate in statement, handy in form, and ready of reference"; and the results hoped for are, "that much time may be saved to busy people and much help afforded to students." The plan is excellent, but the execution is scarcely so successful as one would expect from the names which figure on the title page. Mr Lydekker is responsible for the mammals,

reptiles, amphibians, fishes, and cyclostomes; Dr R. Bowdler Sharpe for the birds; Mr Garstang for amphioxus and balanoglossus; Mr W. F. Kirby for the arthropods; Mr B. B. Woodward for the molluscs; Mr Bather for the lamp shells and starfishes; Mr Kirkpatrick for the moss animals; Mr Pocock for the "worms"; and Mr and Mrs Bernard for the coelenterates and the protozoa. There is, of course, a good deal of excellent work in the book, which is a marvel of cheapness; but some of the sections dealing with vertebrata read as if they were made up of popular newspaper articles, hurriedly, and not very skilfully, welded together. The best part of the book is that dealing with the lowest vertebrates and the invertebrates. The sections on lamp shells and starfishes deserve special mention; and students of the bryozoa will be grateful to Mr Kirkpatrick for appending to his section a classification and bibliography. In a second edition it would be well to adopt the same zoo-geographical regions for mammals and birds; and the puzzling sentence on p. 122—"the teats of the female elephants are placed between the hind legs, and the young calf sucks with its mouth, and not with its trunk"—should be deleted. Stricter supervision, too, should be exercised over the illustrations. Fig. 86 (p. 156) bears the inscription *Tragelaphus angasi*, about which no word occurs in the text; the inscription of Fig. 82 (p. 349) does not refer to the bird figured; the illustration of the bearded reedling (p. 368) bears the generic name *Calamophilus*, while *Panurus* is given in the text, though it does not appear in the index. The misprints, of which there are considerably more than are justly chargeable to the printer, should be carefully sought for and corrected. *Alunda*, *Teirao*, *Phasiandae*, *Paro*, *Syrrhoptes*, *Scolopaeinae*, *Nyctierax nyctierax*, *Trypanns*, *Anthrophysa* (and many others) are likely to prove hindrances rather than helps; and some readers may stumble at "catenanan formation." "Pellage," too, is an unusual form in English books; while "Leydecker" and "Brydden" conceal familiar names.

MOTHS

A HANDBOOK TO THE ORDER LEPIDOPTERA. By W. F. Kirby, F.L.S., F.E.S. Vol. V. Moths. Part 3. 8vo, pp. 332, plates 32. (Allen's Naturalist's Library.) London: W. H. Allen & Co., 1897. Price, 6s.

WITH praiseworthy celerity, Mr Kirby has brought his handbook of lepidoptera to a conclusion. It is unfortunate that his account of the noctuids, the geometers, and the whole of the so-called "microlepidoptera" has had to be compressed into the volume now before us. The space is quite inadequate for a due treatment of these groups, especially as the author continues to devote a quarter or half a page to the synonymy and references of each species which he selects for description. Although a large number of moths are described and figured, the families are necessarily much more cursorily treated than those dealt with in the preceding volumes. For example, among the noctuids we find only one British species, each of such large genera as *Acronycta*, *Leucania*, and *Agrotis*, and not a single representative of *Hadena*; and turning to the geometers, the large and important genera *Eupithecia* and *Cidaria* are altogether omitted. As for the "microlepidoptera," Mr Kirby states in his preface that he has found

it "impossible to do more than describe and figure a selection of species belonging to various families."

It is a considerable disappointment to find that in his classification of the noctuids and geometers, Mr Kirby closely follows the arrangement proposed forty years ago by Guenée, instead of availing himself of the work of those modern writers who have critically studied the structure of these moths. There is probably hardly a serious student of the noctuids who would not closely associate the genera which Guenée distributed between his two "families," Apameidae and Hadenidae; yet Mr Kirby treats these assemblages as sub-families, and separates them widely in his series. It is sincerely to be hoped that in the coming volumes of his great catalogue of the lepidoptera Mr Kirby will adopt a more modern classification. Otherwise the value of his work will be seriously diminished.

It could not be expected that much space would be devoted to the habits of the moths which are mentioned, but a few notes of considerable interest on this subject are given by Mr Kirby. He has rescued from a long obscurity an account published in 1830 by the Rev. L. Guilding on the aquatic larva of a West Indian pearl-moth (*Petrophila fluviatilis*). He also calls attention to de Riville's account, published nearly one hundred and fifty years ago, of a Mediterranean *Antispila*, whose caterpillar mines in vine leaves.

A large number of species are figured in the coloured plates, a good proportion of them for the first time. Though the effect of some of the colouring is rough, and there is a want of uniformity in the setting of the specimen, these figures will be helpful for the determination of species. Mr Kirby's wide knowledge of insects and their literature must needs make his writings useful to naturalists, even if they do not care to adopt all the changes in well-known names which he believes to be necessary.

G. H. C.

A BIBLIOGRAPHICAL ENIGMA

MANUEL DE GÉOGRAPHIE BOTANIQUE. Par Oscar Drude. Traduit par Georges Poirault et revu et augmenté par l'auteur. Livraisons, 14-16. 8vo, pp. 513-552, with 4 maps. Paris: Klincksieck, 1897.

WE are glad to receive this, the completion of a useful translation and edition of Drude's work on plant geography. The previous parts have been noticed as they appeared; the last consists chiefly of an exhaustive, and so far we have tested it, accurate index, a list of additions and corrections, and four folding maps. The price of the book as a whole is 18 francs. While commending the work, we must call attention to a serious omission, from a bibliographical point of view. The title page, just issued, bears date 1897; but the first part appeared in June 1893, and the remainder at various dates between 1893 and 1897. As the covers of the individual parts all bear the same date, viz., 1893, and as there is no reference to successive dates in the text, its issue in parts will be lost sight of, and the whole will seem to have appeared in the present year. Thus another puzzle will be added to the future bibliographer's list—a list already far too long. To lessen this evil as far as possible we append the dates of

notice in *Natural Science* of the individual parts. As *Natural Science* is wont to be prompt, this gives a very fair indication of the dates of publication:—

Part 1,	noticed in vol. iii.,	p. 152 (Aug. 1893).
Parts 2 and 3,	noticed in vol. iv.,	p. 464 (June 1893).
" 4 and 5,	" vol. vii.,	p. 214 (Sept. 1895).
" 6 and 7,	" " "	p. 282 (Oct. 1895).
" 8 to 10,	" vol. viii.,	p. 62 (Jan. 1896).
" 11 to 13,	" " "	p. 393 (Dec. 1896).

NEW SERIALS

WE learn from *Science* that the Italian Società Positivista has established a bi-monthly journal, *Il Pensiero Moderna*, published at Via Collegio Romano 26 (Rome?), and edited by Prof. Sergi. The object of the society is to demonstrate the importance of science for modern life.

The *American X-ray Journal* is edited and published by Dr Heber Roberts, St Louis, Mo., and is intended for the medical profession.

We may mention here the *Bulletin* of the Geological and Natural History Survey of the Chicago Academy of Sciences. The first is a monograph of the lichens of Chicago and the neighbourhood by W. W. Calkins.

FURTHER LITERATURE RECEIVED

FIRST Principles of Natural Philosophy, A. E. Dolbear: Ginn, Boston. The Choniostomatidae, H. T. Hansen: Host, Copenhagen. Euclid (Books i.-iv.), and The Tutorial Trigonometry: Clive. System der Bakterien, W. Migula: Fischer, Jena. Untersuchungen über das Erfrieren der Pflanzen, H. Molisch: Fischer. Allgemeine Physiologie, M. Verworn, ed. 2: Fischer. Open-Air Studies in Botany, C. Lloyd Praeger: Griffin. Elementary Biology, T. Jeffrey Parker, ed. 3: Macmillan. Twenty-seventh Ann. Rep. Entom. Soc., Ontario, 1896. U.S. Dept. Agriculture, Technical Series, No. 6. Ann. Rep. Manchester Museum, 1896-97. Journ. Inst., Jamaica, vol. ii., No. 4. First Ann. Rep. Geol. Commiss., Cape of Good Hope, 1896.

Crustacea of Norway, G. O. Sars. vol. ii., pts. v., vi.: *Bergens Mus.* The Asparagus Beetles, F. H. Chittenden: *Year-Book U.S. Dept. Agric.* Insect Control in California, C. L. Marlatt: *ibid.* The Use of Steam Apparatus for Spraying, L. O. Howard: *ibid.* The Protective Value of Action, volitional or otherwise, in Protective Mimicry, F. M. Webster: *Journ. New York Entom. Soc.* Biological Effects of Civilization on the Insect Fauna of Ohio, F. M. Webster: *Ann. Rep. Ohio State Acad. Sci.*

Jersey Weekly Press, August 7; Amer. Geol., August; Amer. Journ. Sci., August; Amer. Nat., August; l'Anthropologie, May-June; Botan. Gazette, June-July; Feuille des jeunes Nat., August; Irish Nat., August; Literary Digest, July 10, 17, 24, 31, August 7; Naturae Novit., No. 12, June; La Naturaleza (Madrid), Nos. 20-22; Naturalist, August; Nature, July 22, 29, August 5, 12; Naturen, June, July; Photogram, July, August; Review of Reviews, July, and do. Australia, May; Revista Quind. Psicologia, &c., vol. i., fasc. 1-7; Rev. Scient., July 17, 24, August 7, 14; Science, July 9, 16, 23, 30; Sci. Amer., July 10, 17, 24, 31, August 7; Scot. Geogr. Mag., August; Scot. Med. and Surg. Journ., August; Proc. Biol. Soc., Washington, vol. xi., pp. 213-230 (July 15).

OBITUARIES

SIR JOHN CHARLES BUCKNILL, one of the first editors of *Brain* and editor of the *Journal of Mental Science* for nine years, was, at the time of his death recently, acting in the capacity of Censor, Councillor and Lumleian Lecturer in the Royal College of Physicians. In 1866 he was elected Fellow of the Royal Society, and in July 1894 he was knighted. He produced a large number of psychological works, making insanity and similar subjects a specialty. He became especially popular through his psychological essays on the "Mad folk of Shakespere."

MR SAMUEL LAING, who died on August 8th at the advanced age of 87 years, was formerly chairman of the Brighton Railway and had a lifelong connection with railway interests. He devoted his leisure to scientific pursuits, and his principal original work was the exploration of the prehistoric refuse heaps of Caithness, which he described, with the aid of Prof. Huxley, in 1866. During recent years he successfully devoted himself to the popularisation of science, his best known works being entitled "Modern Science and Modern Thought" and "Human Origins."

THE death of Captain BERTRAM LUTLEY SCLATER at Zanzibar on July 24th will excite widespread sympathy among English naturalists for his father, Dr P. L. Sclater, as well as deep regret at the loss of an officer whose career was full of promise. His main work was road-making in British Central and British East Africa; during which he accomplished many careful surveys. His maps form a valuable addition to our knowledge of the geography of those countries, in the future development of which the work which cost him his life will play an important part.

ANOTHER geographer whose death cannot pass unnoticed in *Natural Science* was the late NEY ELLAS, a man whose work, though popularly very little known, was of such importance as to place him among the greatest English travellers of this century. His first paper, "Notes of a Journey to the New Course of the Yellow River," is one of the classics of physical geography. His exploration of western Mongolia during a journey from Pekin to Nijni Novgorod is one of the six great feats in Asiatic travel. In 1885 he settled the vexed question as to the sources of the Oxus, and later on made numerous less famous journeys in the Indian borderlands. His shyness was excessive, and he had no ambition for notoriety. His great feats are recorded in technical geographical papers, but these will live. His reputation as a traveller will probably be greater in a century's time than it is to-day. But in the meanwhile it would be very useful if his papers were collected and republished with some sketch of his life.

THE death is announced of THEOPHILE CHUDZINSKI at Paris on June 18th, aged 55. By birth a Pole, he studied at Moscow until the insurrection of 1863 caused him to give up his studies and join in the movement. This was followed by an incarceration of several months in Austria, but escaping he made his way to Belgium and subsequently to France, where he spent the rest of his life. It was in pursuing his anatomical researches that he was first noticed by Broca at Paris, who later gave him a post in the Laboratoire d'Anthropologie des Hautes-Etudes. For several years Chudzinski assisted his master until the latter's death, when he devoted himself to anatomical works, particularly to the study of the brain and the anatomical resemblances and differences between that of man and of the anthropoid apes. A large number of anthropological and anatomical works were the result of his minute researches. These, although edited in Paris, were not published in French.

LUCIEN BIART, who died recently, was a talented author, and although he chose to veil his scientific knowledge in the form of novels, that knowledge was incontestable. A great love of travel took him in 1845 to Mexico, where he studied archaeology and ethnography. In addition to his novels, the chief of which are "Le Roi des Prairies," "Entre deux Océans," etc., he wrote a volume on the red races for the *Bibliothèque ethnologique*, as well as a monograph on the Aztecs.

The deaths are also announced of:—PAUL SCHUTZENBERGER, the physiological chemist of the College de France, Paris, aged 67; P. C. PLUGGE, Professor of Pharmacology and Toxicology at Göttingen; ARMINIO NOBILE, Professor of Geodesy, and author of many valuable papers on astronomy, at Rome; Professor OERTEL of Munich, distinguished for his researches on the etiology of diphtheria; ALFRED MOQUART, Professor of Anatomy at Brussels, on June 5, aged 42 years; MARTIN WILCKENS of the Agricultural School of Vienna, on June 10, aged 64; Count VICTOR TREVISAN DI SAN LEON, the cryptogamist, in Milan, on April 8, aged 79 years; ROBERT DOUGLAS, known for his work in arboriculture and forestry, on June 1, at Waukegan, Ill., aged 84; G. OSSOWSKI, the geologist, on April 16, at Tomsok; P. B. L. VERLOT, botanist, at Verrières-les-Buisson; Rev. ROBERT HUNTER, botanist, on Feb. 25, at Epping Forest, aged 74; SAMUEL JAMES AUGUSTUS SALTER, botanist, on Feb. 28, at Basingstoke, aged 72; Geheimrat HEYDENREICH, student of Lepidoptera, on May 18, at Osnabruck; the coleopterologist, DANIEL MÜLLER, on May 22, at Barcelona; the oologist, C. Q. ASCHAN, schoolmaster at Kuopio, Finland; Dr ANDERS JOHAN MALMGREN, a well-known ichthyologist and student of Annelida, of Uleaborg, Finland; Dr WÖLFERT and a mechanic named KNABE, who fell while sailing at a height of 1000 feet in a navigable balloon, at Tempelhof, near Berlin; FERDINAND BÉCLARD, palaeontologist at the Brussels Museum, who was in the midst of important studies of Devonian brachiopods; R. ALLAN WIGHT, the economic-entomologist of Paerua, near Auckland, N.Z., on the 22nd December 1896, aged 73 years; MICHAEL ANGELO CONSOLE, professor in Palermo University, and well-known as a cactus-hunter, on May 13, aged 85; PETER VON TUNNER, of the mining district of Leoben, on June 8, aged 89 years; DOMINIK HOFER, the veterinarian of Munich University, on June 13, aged 80; Dr JULES JULLIEN of Havre, the zoologist (Bryozoa); CHARLES F. WELLS and J. W. JONES, who were exploring the West-Australian deserts, killed by the natives in June; FRIEDRICH C. STRAUB, the botanist, at Liberia, on March 21, aged 26; ALFRED SUTTON, of the well-known firm of J. Sutton & Sons, Reading, on August 9, aged 80.

NEWS

THE following appointments are announced :—

Henry Charles Williamson, as Naturalist to the Fishery Board for Scotland ; W. W. Watts, of the Geological Survey, as assistant-professor of geology in the Mason College, Birmingham ; Miss Bertha Stoneman as professor of botany in the Huguenot College for Women at Cape Town ; Dr Edward Fischer to succeed his father, Prof. L. Fischer (retired), as professor of botany in the University of Bern ; Dr Julius Paoletti of Padua to be professor of natural history at the Melfi Technical Institute ; Dr Pio Bolson as second assistant in the Botanical Garden of Padua ; Dr M. Raciborski of Cracow and Dr Zehntner of Pasoeroean (Java) to be professors of botany and entomology at the experimental station for sugar production at Kagok Tegal (Java) ; Hugo Münsterberg of Freiburg as professor of psychology in Harvard University ; Dr Antonio Crocchia as professor of biology in the Catholic University of Washington ; Dr H. Fling to the chair of biology, F. A. Mitchell to the chair of geography, and Dr F. D. Sherman to the chair of psychology at the Oshkosh Normal School ; Ernest B. Forbes as assistant state entomologist in Minnesota ; Dr C. E. Beecher as professor of historical geology at Yale University ; Dr L. V. Pirsson as professor of physical geology in the Lawrence Scientific School ; Dr Geo. B. Shattuck as assistant in geology at Johns Hopkins University ; Oliver L. Fassig as instructor in climatology, and Dr Charles R. Bardeen as assistant in anatomy at the same University ; Dr Albert Schneider as professor of botany, pharmacognosy and materia medica in the School of Pharmacy of the North-Western University, Chicago ; E. B. Copeland, of the University of Wisconsin, to be assistant-professor of botany in the University of Indiana ; Dr G. J. Pierce to be assistant-professor of botany in Stanford University ; Henry Kraemer to the chair of botany and microscopy in the Philadelphia College of Pharmacy ; Cleveland Abbe, jun., as a fellow in geology of Johns Hopkins University.

SIR FREDERICK MCCOY has retired from his professorship in the University of Melbourne.

MR A. W. BENNETT has succeeded Prof. Jeffrey Bell as editor of the Journal of the Royal Microscopical Society.

MR C. DAVIES SHERBORN has resigned the secretaryship of the Geologists' Association of London, and will be succeeded in October by Mr Percy Emary.

MR C. W. ANDREWS should have arrived at Christmas Island before this number appears. He left Batavia about the 21st July on Mr Ross's schooner.

THE Irish Field Club Union held their annual excursion in July around the north coast of Antrim, making Ballycastle Bay their headquarters.

MR R. T. GÜNTHER is on his way to Lake Urumiya on the Persian frontier, to study the fauna of that lake. We regret to hear that he has had a temporary breakdown in health, and hope he will soon recover and proceed on his way.

WE learn from the *Revue Scientifique* that it is proposed to found an experimental station in Madagascar, for the introduction of European cereals and the improvement of local vegetable produce.

LADY HUMPHRY, widow of the late Sir George Humphry, Professor of Surgery at Cambridge, has presented her husband's library to the surgical department of the university.

ON July 8th, the Geographical Institute of Lisbon, founded in commemoration of the 400th anniversary of Vasco da Gama's departure for the Indies, was opened by the Geological Society of Portugal.

THE German Botanical Society begins its annual meeting at Brunswick on September 21 at the same time as the German Association of Naturalists and Physicians. There will be an exhibition of scientific apparatus.

PROF. GUSTAV BORN, of Breslau, has received the Sömmering prize from the Senckenberg Society of Natural History at Frankfurt for his investigations on the growth of the larvae of amphibia.

IN the absence of Prof. Bütschli, Prof. V. Carus presided over the annual meeting of the German Zoological Society at Kiel, June 9-11. There were present thirty-seven members and thirteen guests. The next meeting will be held at Heidelberg at Whitsuntide, 1898.

DR HENRY WOODWARD, keeper of the department of geology in the British Museum (Natural History), has been permitted by the Treasury to retain his office for another two years. According to the rules of retirement in the Civil Service, his term of service would have expired next November.

THE Darwin statue at Shrewsbury was duly unveiled, and stands in front of the Free Library. It is the work of a Shrewsbury man, Mr Horace Mountford, is said to be an excellent likeness, and is the gift of the Shropshire Horticultural Society.

PROFESSORS D. T. Macdougall and Campbell, representing a Commission from the American Universities, have visited Jamaica with a view of founding there a botanical research laboratory. Other Commissioners have gone to Trinidad. On their return to the United States they will compare notes as to the best locality and come to a decision.

MR GEORGE MURRAY and Mr V. H. Blackman have returned from their trip to Panama, after a successful and profitable voyage. They have obtained a large quantity of plankton containing many new specimens, which will shortly be worked out, and have made numerous interesting observations on living forms. They spent two or three days in Jamaica on the way.

ACCORDING to *Science*, Mr R. W. Porter and Mr A. V. Shand, who are with Lieut. Peary, expect to pass the winter in Baffin Land for the purpose of ethnological and zoological studies and collections. In the summer of 1898 they hope to travel further north and to return to Aberdeen on a whaling ship from Cumberland Sound.

THE first meeting of the Jersey Natural Science Association was held on August 5, Dr A. C. Godfray in the chair. The attendance was small but enthusiastic, and included many well-known names. We wish the Association every success, but hope they will not find the usual trouble arising from the proposed library and museum.

AMONG those visiting Russia during the meeting of the Seventh International Geological Congress at St Petersburg are :—Dr John Ball, Mr L. Belinfante, Mr F. A. Bather, Prof. J. F. Blake, Mr J. H. Cooke, Mr P. Emary, Mr L. Fletcher, Sir Archibald Geikie, Mr Upfield Green, Mr G. F. Harris, Dr Frazer Hume, Prof. M'Kenny Hughes, Mr Philip Lake, Mr D. A. Louis, Mr Henry Louis, Prof. Sollas, Dr P. L. Selater, Mr G. A. Stonier, Mr J. J. H. Teall, Prof. H. G. Seeley, Mr H. Bauerman, and Dr Wheelton Hind.

THE British Museum (Natural History) has acquired the Savin collection of vertebrate remains from the Norfolk forest-bed and other deposits of that coast. A collection of gault fossils from the 300 feet level of the shaft of the Dover coal-field has also been received, and we understand that the whole of the remains from this very interesting and important shaft will be preserved for the national collections, as a typical reference series for the underground geology of the S.E. of England.

THE new Botanical Garden of New York will be on an imposing scale, rivalling the new Zoological Garden which Dr Sclater recently described in these pages (*Natural Science*, vol. xi., p. 36). The coniferous trees will occupy thirty acres, the deciduous trees more than seventy acres; the space for the herbaceous plants will be not less than eight acres, while the bog-plants alone will cover five acres. The area of the lakes and ponds will be six acres. The museum will have a frontage of 300 feet, with two wings, each 200 feet in length.

WE learn from *Science* that an important change has been effected in the administration of the U.S. National Museum. Acting upon the advice of Hon. Chas. D. Walcott, at present assistant-secretary of the Smithsonian Institute, three sections have been formed—the section of anthropology, with Dr W. H. Holmes, of the Field Columbian Museum of Chicago, as head curator; that of geology, with head curator Dr George Merrill; and of biology, with Dr Frederick W. True as curator.

HARVARD UNIVERSITY has received under the will of Mr A. W. Thayer \$30,000 as an endowment fund to assist poor students. University College, Liverpool, receives £7000 as bequest from Mrs Gee for the advancement of the medical department. It has been decided to institute a Robert Gee fellowship in anatomy of £100 a year and four entrance scholarships of £25 each for one year. Yale University receives land valued at \$25,000 by the will of Dr J. T. Atwater of Poughkeepsie; and the Ohio State University an estate left by the late Mr Henry F. Page.

THE following have received awards from the Academy of Sciences at Berlin to assist them in their researches:—Prof. Engler, 2000M. (for African botany); Dr R. Hesse, 500M. (eyes of lower marine animals); Prof. H. Hürthle, 850M. (muscles); Prof. Cohen, 1500M. (meteorites); Dr G. Lindau, 900M. (lichens); Prof. R. Bonnet, 800M. (for a work on blood-vessels); Dr L. Wulff, 1500M. (artificial crystals); Dr Lühe, 2000M. (fauna of North African salt lakes); Prof. F. Frech, 1500M. (geology); and Dr G. Brandes, 300M. (Nemertina).

THE *Lancet* announces that Prof. Engelmann, the successor of Dr du Bois-Reymond as Professor of Physiology at Berlin, is about to make some alterations in the Institute. Of the four departments, those for microscopical and biological work and for chemical physiology will continue with their present directors. Prof. Engelmann intends to enlarge the department for special physiology, and to share the work of direction with Dr Hermann Munk. The department for physical physiology will for the future be known as the Department for the Physiology of the Sensory Nerves. Prof. König, director of the last-mentioned department, will lecture upon the sensory organs during the last four weeks of summer, and Prof. Thierfelder, of the department for chemical physiology, during the first four weeks of winter, on physiological chemistry.

THE Fifty-Eighth Annual Report of the Royal Botanic Society shows a much more favourable prospect. The lease of the Gardens in Regents Park has been renewed for a further term of twenty-one years. The Council has decided to open a school of practical gardening, granting certificates to gardeners, and the material

for practice is assuredly ready to hand. This new school has been officially recognised by the Technical Education Board, who have voted £100 to the Society in aid of the scheme. The Council further intends to establish an institute for the teaching of botany and for promoting original research; but as this is a bold and ambitious scheme, outside aid will be necessary to give it a practical effect.

M. HENRI DE LA VAULX, now travelling in Patagonia, has written to the Société de Géographie de Paris from Rawson, the capital of Chubut. He has visited the Monsonero Indians, where he has found a *tolderia* 18 leagues south of Keurskeule. The cacique, Sayhueke, received him with great cordiality, and he witnessed a *komaruko*, a religious fête. M. de la Vaulx has made an ethnographical collection and taken some photographs. On the shore of Lake Colhue the explorer discovered ancient stone sepulchres, in which he found a skeleton almost perfect, as well as ten skulls of Telhuelche Indians. There were a large number of arrowheads, knives, and stone *boleadores*. These discoveries will prove of great importance to the study of the ancient peoples of Chubut.

In connection with the South-Eastern Union of Scientific Societies, a section for geological photographs has just been established. Its objects are :—(1) To stimulate interest in the observation and recording of geological phenomena; (2) to form annually a set of lantern slides dealing with some part of the geology of the south-east of England, and to circulate these, with an explanatory lecture, among the affiliated societies during the winter session; (3) to form a permanent collection of geological slides and photographs; (4) to contribute to the national collection of geological photographs now being formed at the Jermyn Street Museum under the auspices of the British Association. Particulars as to the work may be obtained from Mr H. E. Turner, the hon. secretary, Bank Street, Ashford, Kent.

THE Bill which the Duke of Devonshire presented to the House of Lords concerning the University of London has been withdrawn for the Session. It proposes to appoint the following Commissioners :—Lord Davey (chairman), the Bishop of London, Lord Lister, Sir William Roberts, M.D., Sir Owen Roberts, Professor Jebb, and Mr E. H. Busk, whose powers continue till Dec. 31, 1898, and whose duty is "to make statutes and regulations for the University of London in general accordance with the scheme of the report" presented by the previous Commissioners, with any "modification which may appear to them expedient after considering the changes which have taken place in London education of a university type since the date of the report, &c." The Bill provides that "after the expiration of the powers of the Commissioners, the Senate of the University shall have power to make statutes and regulations for altering or supplementing any of the statutes or regulations made by the Commissioners." The Senate, consisting of the Chancellor and fifty-five other members, to be nominated by the Crown and certain learned and public bodies, "shall be the supreme governing body and executive of the University. All University property shall be administered by the Senate, and the Senate shall have the entire conduct of the University and all its affairs and functions, provided always that no religious test shall be adopted, and no applicant for a University appointment shall be at any disadvantage on the ground of religious opinions; no procedure to a higher degree shall be allowed without examination or other adequate test, nor shall any honorary or *ad eundem* degree be conferred unless the Senate, in exceptional cases, think fit to confer such a degree on a teacher of the University; no disability shall be imposed on the ground of sex."

CORRESPONDENCE

WOMEN WITH BEARDS

THE idea of the woman of the future having a beard, noticed in *Natural Science*, vol. xi., p. 2, as set forth by Dr A. Brandt, is scarcely new. If it is not definitely stated by Darwin in his "Descent of Man," at any rate it is an obvious conclusion from what he has to say concerning the appearance of distinctive masculine characters, such as horns, sometimes in the male sex only, sometimes in both sexes. The appearance of the beard in *Homo* is quite analogous to that of horns in other animals; and just as horns have apparently been acquired by the females of certain species by what may be called "inherited transference" from the males, so will beards be obtained in time by the future females of *Homo*.

I thought I had actually drawn attention to this matter of beards in "Some Laws of Heredity" (*Proc. Cotteswold Field Club*, vol. x., 1892), but I cannot find it. I had it and the case of horns, etc., in my mind when I wrote therein (p. 275), "A marked character of the male sex . . . being transmitted in accordance with the law of earlier inheritance, ultimately appears early in life in the male. Then the character tends to appear in the female sex also, though why it does so is not clear." Also the transference may be from female to male, which would appear to be the case with rudimentary mammae in the male of *Homo*.

Let me point out another biological aspect of the case:—Facial hairiness is exhibited more by the unmarried than by the married women. It seems that each woman receives from her male parent latent beard-characters. If she have children she certainly transmits such characters to them. If she has no offspring it seems that the characters tend to develop in her own person. So it will be in the old woman, and not in the "new woman" of the future, that the beard will be most prominent—a startling retribution that the most masculine characters should appear in those who are the greatest old maids.

CHELTEMHAM.

S. S. BUCKMAN.

CHEMISTRY IN MUSEUMS

IN his notice of the "Report of the Proceedings of the Museums Association," 1896 (*Natural Science*, vol. xi., p. 132), Dr R. H. Traquair, after referring to my paper on "Chemistry in Museums" as carrying "the educational theory of museums to a pitch of absurdity," goes on to say: "A collection of metals, salts, &c., is no doubt a desirable feature in connection with the chemical department of a school or college, but you will learn chemistry only in the laboratory, and certainly not in a museum." The phrase "pitch of absurdity" is too often on the lips and on the pen points of scientific men, and coming from the quarter whence it does is only a too effective means of killing suggestions which might possibly lead to beneficial improvements. As to the sentence I have quoted, one might generalise in the same heedless fashion about any of the sciences which museums seek to illustrate. But it is not intended in these institutions to supply a complete course of study in any branch of science, but to place such illustrations of them before the public as will be helpful to those interested in the study. Hundreds of cases of stuffed birds and mammals will not teach the science of zoology; all the dried plants and wood sections in the museums of Europe will not teach the science of botany, nor can we learn palaeontology by looking at a fossil *Glyptolaemus* in a museum case. These sciences also can only be "learnt in the laboratory." Would Dr Traquair on that account refuse the stuffed bird or mammal, the dried plant and the fossil, a place in the museum? Do not the many mineralogical cases which litter the floors of museums contain simply "a collection of metals, salts, &c."? and these teach, if they teach anything at all, a very little of the science of chemistry. Is Dr Traquair of opinion that a good artificial crystal of common salt is of less educational value than an indifferent natural crystal of Halite?

At the time when my paper was written I was fresh from the laboratory of a technical college, and thought I saw a way to help not only the laboratory student, but also that larger class which is interested in science but cannot obtain access to the laboratories. I did not, be it observed, advocate the formation of a new museum department; I merely asked for the re-arrangement and extension of a department already existing in some museums. In the Edinburgh Museum of Science and Art, for instance,

there is, as far as case-room is concerned, a very fair section devoted to the illustration of chemical science, but the arrangement followed is so antiquated, and the specimens are so few—many of the cases are almost empty—and so little representative, that the whole thing is distinctly farcical.

It seems to me that men like Dr Traquair, looking down from a great height on the unscientific rabble, are too much in the habit of dividing the population into three classes—scientific men, who have little use for museums; science students, who have their manuals and their laboratories; and the general public, who have to be amused with stuffed animals and big crystals. They forget that large and increasing class, who are the main support of such papers as *Knowledge*, *Science Gossip*, and even *Natural Science*, who look, and have a right to look, to museums to illustrate their reading. It is for this class that museums must cater more and more. The museum of the future, if it is to be of any educational value whatever, must become the laboratory, the technical college, the university, of those who have to earn a livelihood by their hands. Consequently the formation of such a collection as I advocated, were it only in view of its importance as an adjunct to the evening lecturer, is an absolute necessity.

KELVINGROVE MUSEUM, GLASGOW.

GEORGE W. ORD.

SEVERAL correspondents have noted an unfortunate misprint which escaped correction in the June number (vol. x., p. 427, line 34), where "a cross between *Myxine* and the cod" ought to read "a cross between the megrim and the cod."

NOTICE.

TO CONTRIBUTORS.—All Communications to be addressed to the EDITOR of NATURAL SCIENCE, at 67 St James' Street, London, S.W. Correspondence and Notes intended for any particular month should be sent in not later than the 10th of the preceding month.

TO THE TRADE.—NATURAL SCIENCE is published on the 25th of each month; all advertisements should be in the Publishers' hands not later than the 20th.

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